THE END OF THE EARLY BRONZE AGE IN THE AEGEAN

EDITED BY

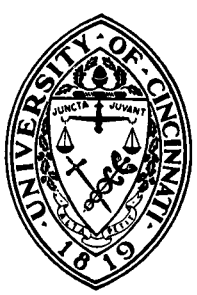
GERALD CADOGAN

THE END OF THE EARLY BRONZE AGE IN THE AEGEAN

CINCINNATI CLASSICAL STUDIES

NEW SERIES

VOLUME VI



LEIDEN
E. J. BRILL
1986

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Contributions by

JOHN L. CASKEY • SINCLAIR HOOD • MARTHA HEATH WIENCKE
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Published with financial support of the Classics Fund of the University of Cincinnati established by Louise Taft Semple in memory of her father, Charles Phelps Taft.

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ΕΙΣ ΜΝΗΜΗΝ

JOHN LANGDON CASKEY

DECEMBER 7, 1908 — DECEMBER 4, 1981

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PREFACE

The essays printed here were given as lectures at a symposium sponsored by the Department of Classics of the University of Cincinnati and held at the University of Cincinnati on March 15-17, 1979.

1979 was the year of Jack Caskey's retirement. The topic of the symposium, "The End of the Early Bronze Age in the Aegean", was fitting for him and as much for his colleagues and students at Cincinnati over many years. Their contributions to our understanding of the Early Bronze Age and the Middle Bronze Age in mainland Greece and the islands continue to stimulate all working in the prehistory of the Aegean and the East Mediterranean.

As editor, I apologize deeply for the slow publication of this book—and may attribute a little of that to the travails of a peripatetic life. The essays are printed as they were sent in between 1979 and 1981, with a few further illustrations coming thereafter, and with references changed for works that were in press and have since been published.

Jeff Clarke has drawn the maps; Tucker Blackburn has helped with other illustrations and with advice; Mary Lou Bradeen has been splendid and patient in typing the manuscripts; and Diane Fortenberry and Ned Fletcher have been invaluable editorial assistants. The contributors would like to thank all of them very much, and also the audience and the members of the department at Cincinnati, whose comments, company and help made the symposium a lively, enjoyable and, we hope, productive event. It was supported by the Classics Fund, a gift of Louise Taft Semple in memory of her father Charles Phelps Taft. We are grateful for her generosity and imagination, as Jack Caskey was. We are sorry he is not alive to see this book.

G.C.

Cincinnati
May, 1983.

PLATES

John L. Caskey, Keos, September 1981.

PLATE 1a. Nsibidi ideograms (R. F. Thompson 1978). (Published with the permission of the author and Yale Alumni Publications, Inc.)

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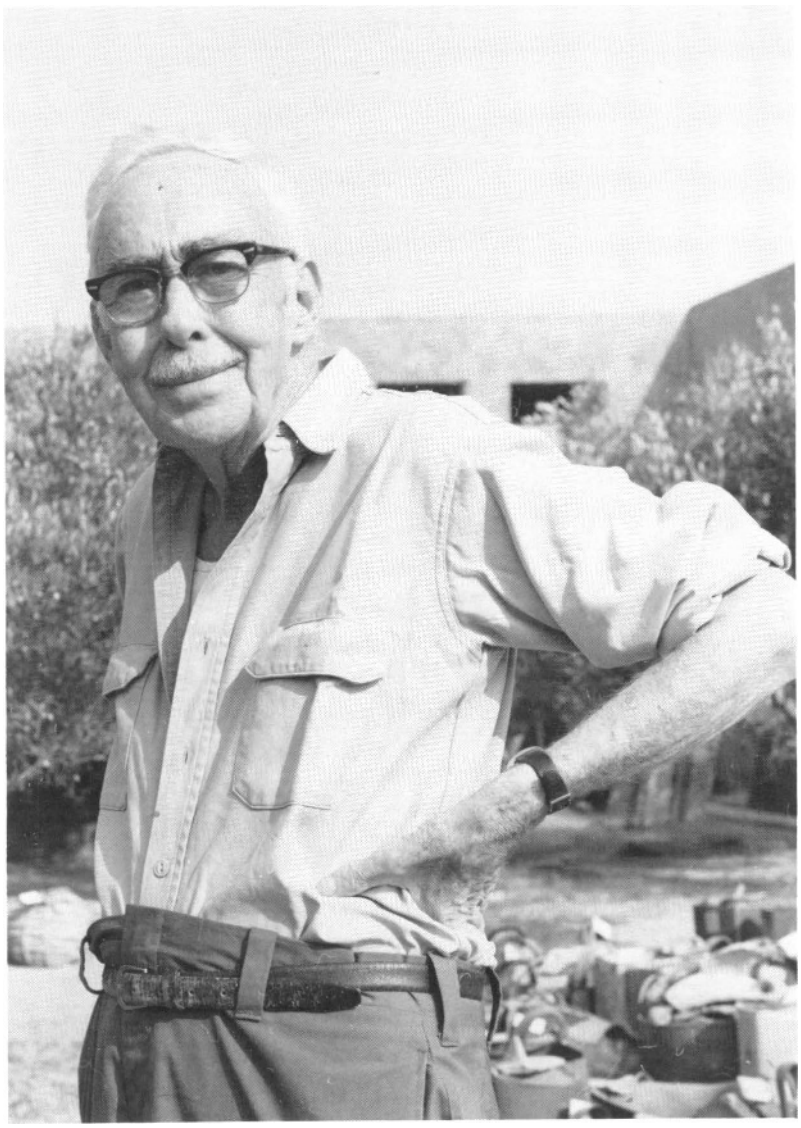

John L. Caskey, Keos, September 1981

JOHN L. CASKEY

Address at the Burial of the Dead and the Holy Eucharist for John Langdon Caskey, St. Michael and All Angels Church, Reading Road, Cincinnati, December 10, 1981.

It is a difficult task in a few days, or even with several years, to know what to say about Jack Caskey. He was a private person, full of surprises, witty, in love with his work and a good friend. Any account of such an archaeologist as Jack will be inadequate, just as any account of archaeology is inadequate, by definition. It may be easiest to begin by reading from the citation, which many of you will not have heard, when Jack was given the sixteenth Award for Distinguished Archaeological Achievement of the Archaeological Institute of America in New Orleans after Christmas last year. (The first Award was given to Carl Blegen.) The citation was written by Eve Harrison and Machteld Mellink, old and staunch friends.

> JOHN LANGDON CASKEY, excavator and interpreter of preclassical Greece, has transformed and enriched our understanding of the preliterate cultures of the mainland and the Cyclades.
>
> In the Argolid he wisely selected the mound beside the Lernaean spring to test our concepts of Helladic prehistory. Working with a team of associates and apprentices, he exposed the House of the Tiles as a clue to Early Helladic art and architecture, economy and organization, with due regard for bio-archaeology. The fate of this mansion, its destruction well before the end of the third millennium B.C., and the rebuilding of Lerna in utterly different form by people of different customs, led Caskey to re-examine carefully, in many *Hesperia* reports and other discussions, the story of the coming of the Greeks ...
>
> At Ayia Irini on Keos, the Cycladic island off the Attic southeast coast, Caskey selected his second major objective with keen insight and instinct. With a team taught through his lucid and methodical approach, he brought to life the Bronze Age town on the bay of Ayia Irini, its growth as an island community in contact with neighbors and rivals, Minoans and mainlanders, and he again made us discern matters of cultural interaction and chronology, of Greeks and pre-Greeks, and this time, also of major art and religion ...
>
> In 1949, Caskey began a decade of distinguished service as Director of the American School of Classical Studies at Athens, seeing the School through the exciting but difficult phase of returning after the disruptions of war to the level of scholarly activity and excellence it had previously enjoyed. In this arduous service he earned the affectionate respect of many Greek and foreign colleagues as well as members of the American School ...
>
> In 1959 Caskey became Head of the Department of Classics at the University of Cincinnati, under whose auspices he excavated in Keos. He maintained the high

reputation this Department had acquired under Blegen, attracting first-class students and giving them first-class training.

In awarding John L. Caskey the Gold Medal for Distinguished Archaeological Achievement the Archaeological Institute of America honors an excavator, author, teacher and administrator who has broadened the great tradition of archaeology and opened new horizons in the early history of Greece and its people.

The citation ends. As a Cincinnatian, Jack was always a devoted citizen of the Queen City, even if he did send new members of the faculty coming from abroad Mrs. Trollope's book to let them know what they were, or were not, in for. He was delighted to find that his grandfather had graduated from Woodward High School before going to Yale—his other grandfather was Episcopal priest in Dresden; and his only blood relation lives near, in Kettering. He valued enormously the training he received here as a graduate student from Blegen and his colleagues, and believed completely in the ideal of our department of studying Aegean prehistory as part of a living classical tradition, and so as part of our Western tradition of civilisation and culture. His concern for the Department was profound and constant.

He was well aware of the support of the Taft Memorial Fund for his own fieldwork, and the research of others in the Department, and grateful, and glad of the support of others in this city. In a small way, this generosity has given the city as distinguished a reputation internationally for classics and archaeology as for machine tools, jet engines or soap powder. Indeed until recently its reputation in Aegean prehistory has probably been greater in Europe than in the United States.

Jack was equally aware of the support needed at the other end, from students, many of whom have a large part in the study and publication of Lerna and Ayia Irini. His students are a distinguished band. He was fond of the young. Even when Jack had little or no formal teaching, he was always eager to hear what the newcomers to the Department were doing, how they were getting on and where they might be going for fieldwork in the summer. We were talking about them the day before he died.

He himself had been in Greece from the age of eight, and had travelled still earlier elsewhere in Europe. Much of the company had been adult, and he always knew how much he had learned from his elders. In this way Jack was a pious man. He also knew languages, as one says in colloquial Greek. Turkish, learnt for Troy, was essential for his work in the O.S.S. during the war—being a classicist and an archaeologist were two other strong recommendations for the intelligence services, on both sides of the Atlantic Pact. His Turkish was found to have its diplomatic value when the President of Turkey was on a state visit to Greece, and Jack was asked to guide him and the King of Greece round the excavations at Corinth. Jack began

in Turkish, with some Greek and English. Everybody was so relieved. At the end of the tour, I am told, Jack was taken aside. "By the way, are you doing anything tonight? Could you join the King and the President for dinner?"—on a destroyer anchored below in the Gulf of Corinth.

His English was beautiful. He loved English and cherished it; and though in many ways at heart a romantic, he treasured its irony, and the ironies of life that that represents and, I am sure he would agree, the ironies of archaeological evidence. We treasure his works of scholarship and their sense of irony—there are few who would quote Thurber, a favourite author, for an article on 'Aegean Terminologies' in *Historia* (Caskey 1978). The carefully placed semi-colons, and the ways of speaking to those who hear and writing to those who read allow for the difficulties of our subject, and the impossibility of learning much of a past which has no written literature if one is didactic, pompous or a worshipper of jargon.

We also treasure Jack's letters, which went to this many friends and acquaintances, of all sorts and interests, around the world. Just as the spirits sink when there is an envelope from the tax people, so they would rise when there was an envelope from Jack. Even if he was depressed, the letter would still be a joy: direct but not without some byways, addressed to the recipient but true to the mood of the writer, thoughtful even in the tiniest matters, always phrased well and unexpectedly, often in another language.

It is difficult to describe Jack. He was very private, witty and wary, and as much an artist as a scholar. His digs were his own creations, three-dimensional sculptures being carved backwards. He loved sailing, dry Martinis, whales and all other good causes, elegant writing, the bold spirit and the gentle put-down, ouzo from Argos and anything else that makes up culture, which is what archaeologists try to discern.

Jack died from amyotrophic lateral sclerosis, "Lou Gehrig's disease". It had obviously been there for the last two or three years, which is why he was so often tired and depressed. After his return from Greece in September, the disease bolted and the prognoses were shorter and shorter, though not so short as what happened. To the end Jack was brave, cheerful and self-deprecating, as those of us who were lucky enough to be with him last Friday know. He never complained. He was buoyed by his love for Miriam and her love for him; and he was thinking of the work that had to be done. It was truly a relief to him that unfortunate misunderstandings with the Department were resolved, last week, when the Department and so the University re-affirmed their commitment to completing the work at Ayia Irini, whatever happens. I should like to read here the last paragraph of his article of 1978 on 'Aegean Terminologies' (Caskey 1978):

> There is indeed nothing sacred about an established terminology, except perhaps an element of quiet veneration which is owed to the intellect and insight of those who devised it. People of that stamp were themselves innovators, not obstructors of progress. The time may come—almost certainly will come, and possibly quite soon—when a fundamental change may be accepted and generally welcomed. Before then, however, some wide gaps need to be filled. Let us record and make known, in words as simple and familiar as the subject matter may allow, the facts already collected and those which can be derived from newly available scientific processes. These are large tasks.

They are large tasks; but much larger was his task of digging and directing the work at Lerna and Ayia Irini. They are both very important excavations. He is a brave man who undertakes two such digs, when a week of digging means months or years of study. The better the site, the longer the study. Jack was a brave man. All of us, in the University and city, in the small world of Aegean prehistory and in the large world of Western civilisation, owe Jack very much for what he has taught us both by his two great excavations and by what he has written. He wanted to finish his tasks, and could not. An archaeologist is humble before the messy evidence, and the work of interpreting it. Jack was, and he knew the need for help from colleagues, whom he saw rightly as his friends in archaeology. When the volumes come out, as they will over the next few years, there will be a monument more lasting than bronze—which all archaeologists know corrodes easily and suffers from bronze disease. Jack will wish that now among all our different prayers we pray for a quiet night and a perfect end to two mighty adventures into the history of man. Amen.

G.C.

THE AEGEAN
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SITAGROI
DIKILITASH
THRACE
VARDINA
MACEDONIA
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SERVIA
AYIOS MAMAS
LEMNOS
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AYIA MARINA
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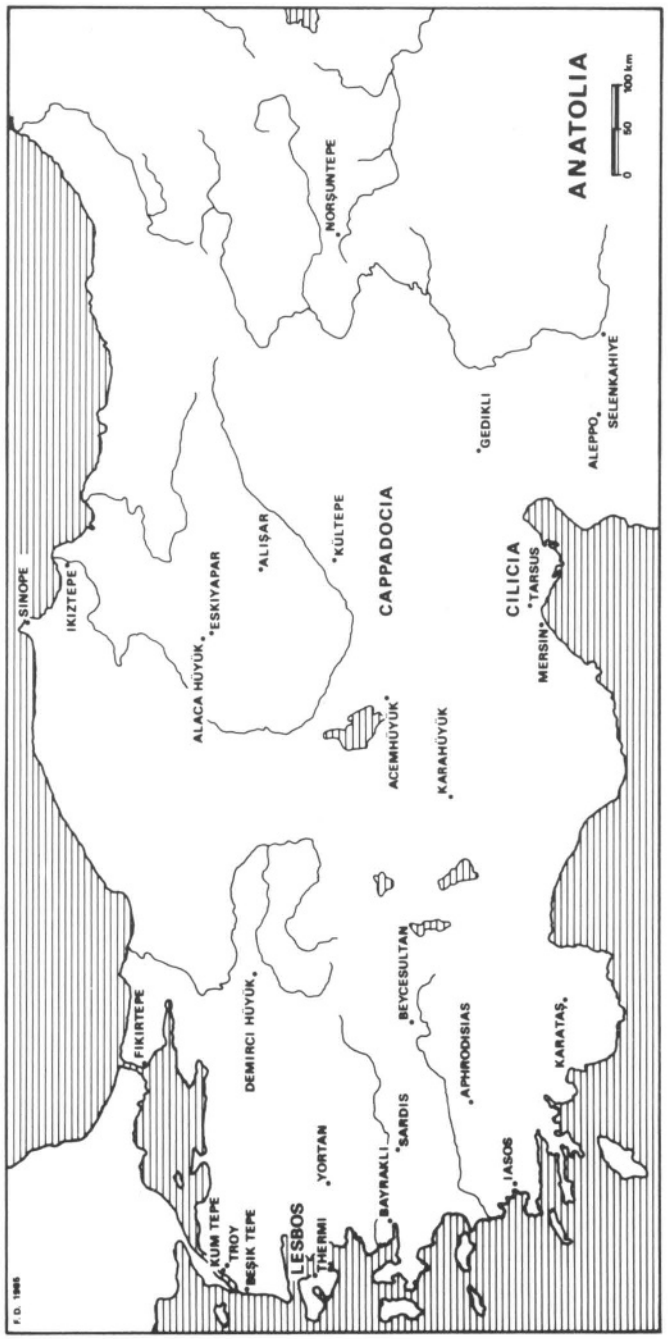
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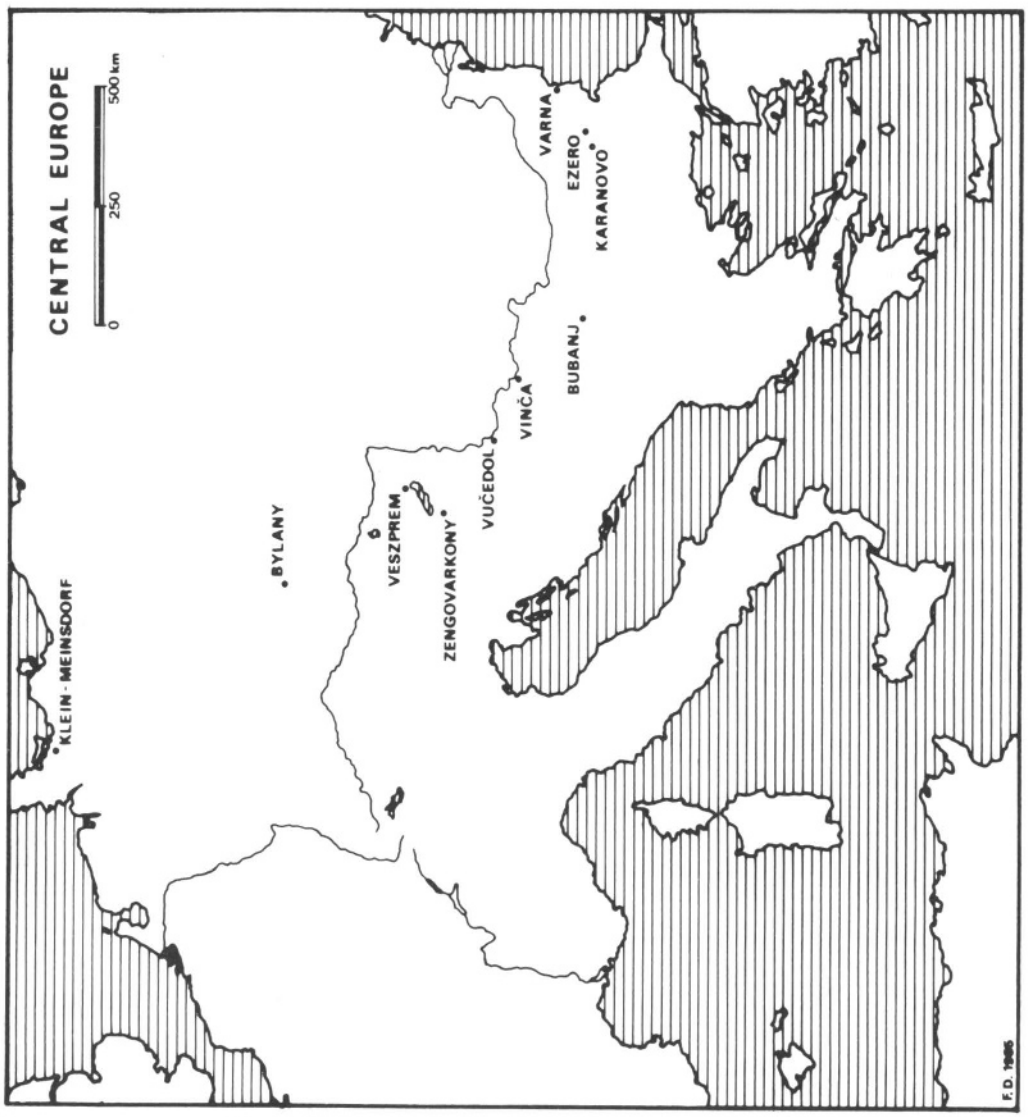
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VARNA
F.D. 1986

DID THE EARLY BRONZE AGE END?

JOHN L. CASKEY

(Plates 1-4)

Let not the title be taken as facetious. It was chosen for brevity, and to imply other questions: What was "The Early Bronze Age?" and what do we mean by "End"? I had hoped to clarify my own thoughts about these questions and to approach some comprehensive answers, but must confess that I have been disappointed. Too many pieces of the great puzzle are still missing, and the meanings of those pieces that lie before us are still elusive. As Professor Momigliano remarked—in another context—when he lectured here recently, the history of Rome is in many ways far simpler, for we know that it began precisely in 753 BC and that it ended in 476 AD.

We in our sphere shall not reach such precision. For the moment, at least, we are limited by other measurements: Early, Middle, Late; Minoan, Helladic, Cycladic; numerical and alphabetical subdivisions; names of so called cultures. I am quite aware of, and applaud, the efforts of those who seek to find more descriptive, accurate and illuminating vocabularies; but, having sat near Sir Arthur Evans and conversed at Troy with Wilhelm Dörpfeld, and learned from Alan Wace and Carl Blegen, I must beg your sufferance if I cling to the traditional nomenclatures (Caskey 1978).

As an introduction to the theme of our symposium let me touch upon a few types of material evidence that are pertinent to the discussion—only a few, since there is no end of them; and with no pretense of thorough treatment.

My thoughts are centered chiefly at places with which I have been most closely associated as excavator: Troy, Lerna in the Argolid, where some of our problems emerged with insistent clarity in the nineteen fifties, and Ayia Irini on the nearby island of Keos, where investigations have occupied many seasons since then. Materials recovered at both sites are abundant—so great, indeed, that many of the necessary studies and publications are still unfinished, my own and those of my busy associates.

Here I propose to review certain of these topics: particularly the central and late stages of the Early Bronze Age (EH/EC II, III), the beginning of the Middle Bronze Age, and the transitions from one to the next. In doing so I shall take the occasion to present some matters of fact, to ask some questions, and to indulge in some speculations.

Literacy

Philologists, philosophers, searchers after the meaning of fragmentary papyri, manuscripts, inscriptions, and pottery, we are all historians—at least in the Herodotean sense of ἱστορίη. Most of those who inquire have words or parts of words to examine. The archaeologist surveying remains of the Early Bronze Age is at a disadvantage: he has no words. But stop a moment: has he not? What do we mean by "words"? Λόγος in the Greek dictionaries occupies many fine-printed columns of examples and explanations. Are we thinking only of words that we can read as in a book familiar to us?

Potters even of the Neolithic period (Vitelli 1971; 1977), to say nothing of the EBA, made scratches, ridges and marks which surely had meaning for the maker, and almost certainly conveyed meaning to people who saw them. The marks are simple in form and few in number, as compared with those of later ages, but they exist. We cannot read them. Do they represent sounds, names, numbers or quantities, places, qualities? At present we can only guess. But let us not be impatient; systematic collection and tabulation of those marks may yet furnish answers. (Those from Lerna are being published by W. E. McLeod, those from Ayia Irini by Aliki Bikaki.)

Then there is another class, often more elaborate in form, cut or painted on the surfaces of pots and other objects, and commonly regarded as "decorative motifs." How many of them were just that? How many may in fact have been symbols, ideograms, that bore an intelligible meaning?

One cannot but think of the spindlewhorls at Troy that quickly caught and held Schliemann's attention. His efforts to understand and interpret their markings are familiar to us all. We tend to smile tolerantly—not only at his own intense enthusiasm but at the theories of distinguished scholars whom he called in (or who volunteered their interpretations uninvited); Émile Burnouf who remarked of one example "*les caractères ... sont parfaitement lisibles en chinois*"; A. H. Sayce, Martin Haug, Theodor Gomperz, Max Müller, and many others. It was the time of the decipherment of the Cypriot syllabary, the early eighteen seventies; quite naturally the learned commentators sought parallels. And when one seeks earnestly enough, one finds.

It is not for me to pursue or criticize that episode. Others here present are far better qualified. I cannot believe with Schliemann that the owl-faces on early Trojan stone idols, pots and implements, are those of Athena Glaukopis, but I applaud his attempt and those of others (perhaps yet unborn?) to discover what they did represent. For I cannot think that the little faces were made and repeated on many different objects unthoughtfully or in ignorance or frivolity.

A seal impression on the rim of a terracotta pan-hearth, in the collection from Ayia Irini in Keos, shows various elements grouped in a composition which may well have had meaning for the beholder: among them a tree-like object, a water bird and, unmistakably, an EBA sauceboat (*CMS*, V, no. 478). From Lerna come clay sealings with spiders, harps(?), and EBA long-spouted jugs; and a pithos band with animals running (*CMS*, V, nos. 115, 110, 109, 120). Did these symbols carry messages?

Let me move for a moment to a topic not unrelated, but far in time and place from that which we are discussing. R. F. Thompson, Professor of the History of Art and Master of a college at Yale, has long studied the black cultures of Africa. Recently he wrote a short semipopular article on an ideographic writing that seems to have originated in the region of Calabar in Nigeria and western Cameroon (R. F. Thompson 1978).

In the nineteenth century, with condescension typical of the imperialist mentality of the west, black Africa was thought of as generally illiterate, though in fact some local languages had been written in Arabic script since the Middle Ages. The script of Calabar, called *nsibidi*, was also far from new at the time when it was brought to the New World by Africans who were caught and carried as slaves and laborers to the sugar ports of Cuba and elsewhere in the Americas. Elements of the language, transported thence and "creolized" as Thompson tells us, can be heard today in Afro-Hispanic speech of the city of New York.

Communication in *nsibidi* is by symbols representing ideas. Many are very simple and expressive: for example, a pair of interlocking curved lines meaning love, unity, marriage (Pl. 1a, 1); the same lines separated, often with an intervening barrier, meaning hatred, disunity, divorce (2); a spiral indicating a tortuous journey, and pairs of parallel lines presumably a straight path (3). Some may be elaborated: the spiral, with adjuncts, may depict a woman awaiting her lover (4) or a man going to his beloved (5). Others have meanings perhaps less obvious to us: plain crosses for "word" or "speech" (6); a comb-like sign for "young men gossiping on the council log" (7); a quartered circle with small circles in the quadrants for "the all-seeing eyes of God (8)."

This writing had its public and social uses, but also its sombre and secret meanings. Ideographs were called the "dark signs." Parts might be blackened when signifying a corpse or a sword or an execution (9-11); and a "darkened relationship" shown by blackening the overlapping part of the marriage symbol (12).

Some again are much more complex: a depiction of events leading to an execution (13); or again, now obviously joyful, a kite-like sign announcing a festival (14).

My information comes only from Thompson's brief article (full treatment of the

subject to follow). I must not elaborate. But you will have seen why my attention was caught by it: for these examples of *nsibidi* suggest very clearly that thoughts about illiteracy in the Aegean Early Bronze Age may call for reconsideration. I am not suggesting that there are here any universal parallels, or that the Aegean signs and symbols be regarded as an established script. The feathered spiral on a pot of Troy II (Pl. 1b; Blegen *et al.* 1950: pl. 407) is lovely in itself, and its symbolism, if any, must not be explained in Cuban terms. The fringed and scalloped edges of the *nsibidi* straight path have no direct association with their counterparts in familiar elements of Early Cycladic and Helladic patterns on pottery (e.g. Pl. 1c, after Müller 1938: pl. 33, 1-3, and fig. 66). Were the latter not inspired by the stitches of embroidery? Surely, many of the apparent similarities arose by chance.

Architecture

In the middle phases of the Early Bronze Age (EH II) there were monumental structures in the Peloponnese: at Lerna a massive circuit wall with towers and gates; at Lerna, Tiryns and Akovitika great buildings that may be called palaces. One must suppose that there were other fortified places and palatial edifices elsewhere in our region, not yet found or not adequately excavated. Farther afield in Aegean lands, one thinks of Aigina, Syros, and Troy II.

Of smaller ordinary houses for the daily life and work of the populace we have many examples. They were roughly rectangular, often contiguous; very rarely with curving walls.

Among the great buildings, the House of the Tiles at Lerna is the best known (Caskey 1958: pls. 30, 31), being the most accessible and relatively the best preserved. Its predecessor, House BG (Caskey 1959: 203, fig. 1) was apparently similar in plan and slightly bigger, as was the one in Messenia (Karageorgha 1971). Each of these comprised a series of large rooms in the central axis, corridors and smaller rooms on either side, and an entrance at one end, if not at both. The House of the Tiles had one or more upper storeys; the others may have had. At Tiryns the huge building was almost certainly round, a tholos, of comparable monumentality (Müller 1930: 80-88), but we know relatively little about it since it is buried under the Mycenaean palace.

Pottery (notably sauceboats) and a few other objects in the ruins show that these buildings were roughly contemporary. Similar architectural features are seen in Mesopotamia from the fourth millennium onward, or even earlier.

We must look for illuminating parallels and possible relationships, but I hesitate to conclude that our Peloponnesian scheme was necessarily derived from one or

another of the older civilizations, although that possibility is surely not excluded. Increasing centralization and urbanization in EH II undoubtedly played a part. Yet it is worth remembering that the rectangular plan in itself is practical: spacious, structurally well braced along either side if the parallel walls are fastened together, convenient for ceremonies, meetings, circulation, and storage of goods. Examples of such buildings have not been found in EH III or MH towns, but essentially the same scheme recurs in much later Mycenaean palaces, and again much later still in Classical Greek temples—to say nothing of buildings comparable in function (including churches) down to our own day.

I would suggest also that we beware of chronological conclusions that are drawn solely from comparisons among simple long buildings which are closed by apses at one end. There was one, at least, in the earliest stratum of the First Settlement at Troy (Blegen *et al.* 1950: pls. 138, 425). The form was very common in the EH III and MH Periods in Greece (Caskey 1965); but also in Geometric times. May it not merely reflect—in many if not all cases—a mobile, nomadic stage of life in which people built their temporary shelters of supple reeds or branches implanted in the ground below and bound together at the upper tips?

Graves and Skeletons

Many graves were found in the course of excavation at Lerna. A full catalogue and account of them has been drawn up by Tucker Blackburn, with contributions by others, and is nearly ready for presentation. For the moment we omit from this list the nineteen graves of post-Mycenaean date. The rest, all in the area of the settlement, are two hundred and fortyfive in number.

The chronological distribution of the latter is of interest: ten are of Early, Middle and Final Neolithic date; nine of EH III; two hundred and eleven of MH; ten of the period of the Shaft Graves at Mycenae; one of LH II and four of LH III. Not one belongs to the great settlement of EH II.

It seems evident that intramural burial was the normal practice in the earliest times and that the whole extensive Neolithic town, of which we touched only a very small part, must surely contain a multitude of graves, in forms of pits and cists. Perhaps the same was true in the Mycenaean settlement also; or parts of the mound may have been reserved altogether as cemeteries. (Before the time of our excavation upper layers had been eroded and we saw relatively few undisturbed deposits of the Late Bronze Age.)

The total absence of EH II graves is remarkable. Most probably there are cemeteries, which have not been found, in the region of Lerna—and also in the

country around other comparable sites that are known today only by house walls and artifacts. A great many EH II sites are on the coasts, and the inhabitants were undoubtedly mariners. Giving free rein to imagination, one might think of burials at sea; but this would appear an awkward solution for numerous contemporary people who lived, for example, in Arcadia; and there are thousands of normal graves around the settlements of seafaring islanders.

In the Fourth Settlement (EH III) a change is apparent. Graves were found among the houses: one cist, one urn, seven simple pits. All contained bones of infants, none of adults. The pattern may have been different in other parts of the town, and therefore we cannot reach secure conclusions; but the very presence of graves on the site marks a real contrast with EH II.

The Middle Helladic settlement called Lerna V, yields a wholly different picture. Here, among the houses, in an area that can be estimated as scarcely one quarter of the MH town, we found two hundred and eleven graves: pits, a few urns, and cists variously built of stones and bricks. Some 35% of the skeletons were of infants, 21% of children, 44% of adults; males outnumbered females among those in which determining features were measurable.

Lawrence Angel in the second Lerna volume gives detailed descriptions and statistics, and adds many comparisons and tentative conclusions (Angel 1971). His grouping of graves in various parts of the site by families and "clans", though necessarily somewhat arbitrary as he points out, is yet reasonable and it leads to interesting observations. All the groups show racial mixtures. But there are demonstrable differences among these clans: in matrilocality and patrilocality of housing and burial, in the form of skulls, in stature of adults, in fertility and fecundity, in the incidence of porotic hyperostosis which is plausibly connected with resistence to malaria, and in other aspects of physique.

Evidence for comparisons between our third period (EH II) and our fourth (EH III) must be drawn from the few other sites where skeletal remains are known. Angel surmises that the former held many racial characteristics of the original inhabitants, Neolithic or still earlier; that new elements of population arrived at Lerna from abroad in EH III and others again in MH; and that all these converging lines account for the mixed racial strains, northern and eastern, which he finds represented.

The extraordinary respect that was shown for the burnt ruins of the House of the Tiles (a building with religious associations?) suggests that some people of EH II may have survived—perhaps young women who in union with the newcomers were to become mothers of succeeding generations; and that combinations of this sort produced the "hybrid vigor" which led ultimately to Mycenaean and later Greek accomplishments.

Animal Bones

Nils-Gustaf Gejvall, at Corinth in 1958, examined the whole large collection of animal bones and shells from Lerna. Single pieces and grouped units made a total of 25,287 specimens. Some of these, the most significant and problematical, were later sent to Sweden, where he continued the research and wrote the book that was published in 1969 as the first volume in the Lerna series (Gejvall 1969).

That book contains a full account of the study, the methods, and the materials, as well as statements of the conclusions that might be drawn—all with proper reservations about limitations and uncertainties. Let me call attention briefly to a few of the items that are pertinent to our inquiry.

Molluscs were taken, presumably as food, in relatively large quantities during the Neolithic periods—naturally enough, at a village beside the sea. Their shells are much fewer in the deposits of EH II, the proportion somewhat higher in EH III and MH though still far below the adjusted sampling of the Neolithic.

At the other end of our series, we may note that the true horse (*equus caballus*) appears first in the MH layer. The other equine, the ass (*asinus asinus*), is attested earlier, in EH II and EH III. In the MH period there is also one example of domestic fowl (*gallus gallus*), the earliest in Europe so far known to Gejvall at the time of writing.

Dogs (*canis familiaris*), swine (chiefly *sus domesticus*), sheep and goats (*ovis*, *capra*), and cattle (chiefly *bos taurus domesticus*) were present from Early Neolithic times through Middle Helladic. But among these animals we do in fact observe a dividing line, a very notable increase from the beginning of EH III; that is, soon after the collapse of the great period that had reached its zenith with the House of the Tiles.

Possibly related in some way is the greater number of bones of the hare (*lepus europaeus*) in EH III and of the red deer (*cervus elaphus*) in MH. One might be led to conclude that hunting became more effective with the greater number of dogs, but Gejvall's measurements of skull fragments inclined him to think that most of these canines were of breeds adapted for guarding flocks and households rather than for the chase.

A large collection of animal bones from Ayia Irini in Keos, approaching half a million pieces, has been studied and is to be published by Jennie P. Coy of the University of Southampton.

Vegetable Matter

Specimens of vegetable matter were collected from all the layers at Lerna, and in 1959 were examined at Corinth by Maria Hopf of the Römisch-Germanisches Zen-

tralmuseum of Mainz. Some were taken to Germany for more detailed analysis. She promptly published a first report in *Der Züchter* in 1961 and a further more extensive study in the *Jahrbuch* of her great museum in 1962 (Hopf 1961; 1962). These articles have not been entirely overlooked by Aegean archaeologists, but I suspect that they may have received less attention than they deserve.

The materials were gathered during seven annual campaigns, 1952 through 1958. Methods and standards of collecting and storing were still primitive, as compared with those that have been developed and applied in Classical lands since then. Further, the archaeological statistics of the site had not yet been worked out in any detail. Miss Hopf assayed more than two hundred samples, most of which were clusters of many pieces found together; the single specimens reached a total around 4000. But at the time of her writing she and others were not well informed of the very unequal amounts of excavation in the various levels and areas that we tested (see Gejvall 1969: ii (foreword)). Therefore conclusions drawn from her statistical records should be modified by these factors.

Let me mention briefly a few items that are related to our present topic; first those appearing in seeds and fruits from deposits accumulated within the settlement. Vetch (*vicia faba*) and two species of barley (*hordeum*) were present from the end of the Neolithic through the Middle Bronze Age—the barley very sparsely in EH II but in relative abundance thereafter. Lentils (*lens* sp.) and peas (*pisum* sp.) are well represented in EH II, not at all in EH III; the lentils recurring, however, in MH; Wheat (*triticum monococcum* and *dicoccum*) is surprisingly scarce: eleven pieces in EH II, forty-six in late strata of EH III and in MH. Figs (*ficus carica*) were present in Middle Neolithic and EH II deposits, flax (*linum* sp.) only in EH II, but neither of them in any later context. On the other hand, seeds of grapes (*vitis vinifera*) appear for the first time—and then in large quantities—in EH III.

A sudden change from the eating of figs in the House of the Tiles to the drinking of vinous toasts in the apsidal huts of Lerna IV would indeed startle us. But further statistics from Miss Hopf's study modify the picture. Latex casts which she made of hollows in potsherds and other bits of burnt ceramics allowed her to identify further vegetable substances that had been imbedded in the clay: among them five grape seeds in EH II contexts and five grains of wheat in those of EH II—(but no more figs). Seeds of grapes have been found elsewhere in contexts of EH II, e.g. at Ayios Kosmas in Attica (Mylonas 1959: 39-40).

Almost certainly chance in the distribution and discovery on the site of Lerna has played its part. Just possibly, the lentils and peas of EH II were largely replaced by barley in the diet of EH III, figs and linseed by grapes. The recorded evidence, though inconclusive, is not to be wholly rejected.

Miss Hopf in her judicious summaries mentions some possible conclusions. The leguminous plants were present from very early times, and their size and forms seem surely to indicate that they were cultivated, not merely gathered. Some of the fruits and vegetables, however, may perhaps have been picked elsewhere in the district and brought to Lerna for consumption, before they were generally grown on the home properties. The statistics do indeed point to significant changes with the advent of the Fourth Settlement in EH III; and the genera and species which thenceforth seem clearly more abundant were very probably of Anatolian origin. The immigrants, she surmises, came to Greece from Asia Minor.

One other note: only two olive pits were recovered at Lerna, and they were in the few small Classical Greek deposits. Miss Hopf wondered whether most were crushed for their oil-bearing kernels and the residue burned as fuel. In any case, thirteen pieces of charred olive wood were counted, along with those of pine, fir, oak, and fewer bits from seven other trees. All seem to have been native, and to have been present since very early times.

Miscellaneous Objects

Among the objects found in all excavations are many that are generally classified as "small" or "miscellaneous." These have been left too often without adequate publication. In the Lerna collection those of the Early and Middle Bronze Ages were described and analysed by Elizabeth Banks in a doctoral dissertation (Banks 1967). Since then she has made a full review of the Neolithic pieces. The bone tools have been examined further by Olga Krzyszkowska. All classes of objects are to be listed in our reports on the successive settlements and specialized studies will follow as required. The same scheme will apply in the case of Ayia Irini, where at present the work is less far advanced. Today let me comment briefly on only a few of the types, with grateful acknowledgement of my debt to Mrs. Banks and other colleagues.

Implements made of durable substances, like stone, may have been useful over many generations, either continuously or through rediscovery, and therefore the contexts in which they are now found may be much later than the dates of original manufacture. (Apropos of such uncertainties, it may be prudent to bear in mind the tale of the New England farmer who greatly prized his precious axe: it had belonged to his great-grandfather and had been kept always in perfect condition over the years, unchanged, needing in all that time only three new heads and eight new handles.) If there are several or many examples, and most are found in one or two successive strata, the chronology is more firmly fixed.

Heavy stone axe heads, pierced vertically for hafting, with a blade at one end and a flat or convex peen at the other, appear first in EH III at Lerna and are equally common in the MH period. This is an implement for practical use—not of the long elegant shape of the famous pieces from Troy II. Close parallels are found elsewhere in the Peloponnese, in western and central Greece, in Thessaly and Macedonia; all probably contemporary, though in some cases the contexts are not certain. They have been reported as rare or absent in the Cyclades, but at Ayia Irini we have found examples in upper deposits of the MBA (Pl. 1d), and some in the LBA. One might guess that the latter were survivals, but it is a notable fact that marks of rotary drilling are often visible in the shaft-holes of these, whereas the perforations of the earlier ones appear quite smooth, perhaps suggesting a different usage. The holes in all cases are about 2 cm. in diameter.

A cylindrical stone core, cut out by a hollow bit, was found in the first MH stratum at Lerna, and similar pieces of MH date at Asine. Last summer I examined sixty four of them at Ayia Irini. Almost all are of local marble, white, gray, or laminated (Pls. 1e, 2a). Six are from MBA contexts, the rest from LBA or from mixed lots near the modern surface. They come from the perforating of tools and from the preliminary steps in hollowing the interiors of stone vessels which were to be finished by chiselling and abrading. Many were quite possibly cut with sand or emery by rotation of tubular sections of reeds like bamboo (Pl. 2b); but the largest, more than 8 cm. in diameter, must have been made by metal collar-drills. Reeds growing near both sites today have inner diameters rarely greater than 2 cm. Evidently this craft thrived in Keos: we have fragments of many marble bowls or cups, as well as many in serpentinite and other rocks which were imported presumably from Crete. For the moment the significant fact is that the technique of drilling appears to have begun, in Keos as on the mainland, in the Middle Bronze Age.

Many of the cores at Ayia Irini were used after extraction as handy tools for grinding of fine substances or polishing of surfaces. Call them pestles. But there is a whole series of objects that were made deliberately for that purpose. Most are small, 3 to 7 cm. in length, narrowing at the waist to an hourglass profile (Pl. 2c, d). Some are beautiful little objects, made of veined and multicolored rock or, occasionally, cut from the thick part of the shell of the bivalve *spondylus* (Pl. 2e). The substance of the latter was recognized at Ayia Irini by Jennie P. Coy. Other pestles of similar appearance have been listed elsewhere among the objects of stone: e.g. Yiriza and Zygouries, Blegen 1928(2): 196, fig. 186 and pl. 22, 19; Troy, Blegen *et al.* 1950: pl. 363, no. 35-518; Ayios Kosmas, Mylonas 1959: fig. 166, 2.

There is much variety among the fifty three pieces recovered at Ayia Irini; the

smallest only 17 mm. long and weighing just over three grams, the largest 18.5 cm. long and weighing nearly four kilograms (Pl. 2f). I am not aware of any others in our area which even approach this great size, but have been told that they are not uncommon in Persia.

Six of normal size were found in EBA contexts at Lerna, three in EH II and three in EH III. At Ayia Irini one or two belong to the time of the sauceboats, eleven or twelve to the succeeding period; and eight, quite possibly survivors, to the MBA. They are plentifully represented in southern Greece (e.g. Blegen 1928(2): 197-98, pl. 22: 13-21) and the Cyclades, present in central Greece, known in Crete and at Troy. Precise stratigraphical records are lacking in many cases, but a fair number belong very probably to the time of EH II. Thus we have here one distinctive and easily recognizable type of object that bridges the gap between the middle and late stages of the EBA.

The multitude of bone objects calls for detailed analysis. Here I would recall only one piece, from Lerna: a narrow plaque with a row of seven hemispherical bosses on one face—again a very distinctive form (Caskey 1954: 22, pl. 9g; 1960: 297; Banks 1967: 459-61). Comparable pieces were found by Schliemann at Troy; and one, somewhat different, by the Cincinnati excavators in a room of the last phase of the Second Settlement. Others come from sites in southern Italy, Sicily, and Malta (J. D. Evans 1956; Biancofiore 1957). The plaque at Lerna was found with the earliest Gray Minyan sherds, in 1953, and was assigned then to the MH period. Later, when we knew more about Gray Minyan and other wares, the deposit was recognized as characteristic of EH III. There has been speculation about the significance and dates of these objects. J. D. Evans explains them very plausibly as aniconic female "oculus idols," belonging to times late rather than early in terms of the Aegean EBA.

Among countless terracotta objects, again let me mention only one kind, those that are shaped like diminutive anchors (Pl. 3a; Caskey 1956: 162, pl. 471-p). Their use is not agreed upon; I should guess that it was domestic or industrial, not mystical. They are widely distributed, not limited to Mediterranean regions. At Lerna they belong clearly to EH III (nine examples; two others were found a bit higher, in MH strata). Mrs. Banks cites examples elsewhere, probably contemporary, but also a fair number, chiefly though not solely in southern Greece, that seem well fixed in EH II. In this case, we may just possibly have evidence of a Peloponnesian invention that was spreading locally, and gradually northward.

Among these samples then, one sees objects of two types (the fine pestles and the anchors) which occur in both middle and late phases of the EBA; a unique piece (the bossed plaque) in EH III; a group (the hammer-axes) that appears in EH III and

continues thereafter; and a technique (drilling) that begins with the Middle Bronze Age. The sampling is of course too small to justify firm conclusions at present, but the topic, or topics, vere clearly call for thorough investigations.

A preliminary report (1978) by Curtis Runnels of Indiana University presents information about millstones that were found in successive levels at Lerna. He is preparing a comprehensive study of these objects in the Aegean area—a subject which, as he observes, has been accorded scanty and "unenthusiastic" notice in most archaeological publications up to now.

Very briefly: the Neolithic stones, of greenish fine-grained conglomerate or sandstone, are generally of ellipsoidal shapes and they are small. In the Bronze Age, there are distinct differences. The use of volcanic rock increases, ovate shapes tend to replace the elliptical, and the average or median size increases progressively (weights: 2.1 kg. in Neolithic, 4 kg. in EH II, 4.85 kg. in EH III, 6.06 kg. in MH). In EH III and MH, water-worn boulders were split and then trimmed to form some of the millstones. In MH, the technique of trimming and finishing is notably superior.

Mr. Runnels concludes that a sharp break in the sequence occurred at the end of the Neolithic period; that a real change is observable at the end of EH II; and that EH III and MH samples, although closely related in type, are yet distinguishable from each other in many instances.

A Note on Statistics

In the excavations of the nineteen fifties we came upon the House of the Tiles, its predecessor BG, and the fortifications; but few other buildings. Modest structures of the same period may have existed elsewhere on the mound, but their remains have not yet been seen. In any case, the settlement in EH II was a palatial and military establishment, quite unlike the relatively simple villages or towns which preceded and followed it. (The great shaft graves of Lerna VI—LH I—suggest wealth and power, but we know almost nothing about other features of the place in their time.)

When considering the significance of statistics within the Lerna collection—numbers, quantities and kinds of objects, animal and vegetable remains, etc.—one must keep these differences in mind. The House of the Tiles (unfinished at the time of its final destruction) will not have been furnished with goods, or have left strata of rubbish, quite like those of Lerna I, II, IV, V, and VII.

Pottery

Since pots and sherds rarely disintegrate, decay, or melt away, and since they are abundant at Neolithic and Bronze Age settlements, they remain for us a prime source of information—in spite of raucous complaints from some "new" archaeologists who would divert our attention toward other materials and criteria (which, granted, have received too little). To be sure, the pots and sherds present one serious obstacle: the very bulk of them hinders quick and thorough analysis and publication. Studies of ceramics form Lerna and Keos are proceeding. Partial results have been made known through preliminary reports in *Hesperia* and other journals since 1953. Full accounts will follow. A few characteristic types are illustrated here (Pl. 3b-f).

At Lerna in EH II, apart from plain unsurfaced wares, we find what I have called "glazed ware"; that is, coated with a substance that fired lustrous black, brown or red (Caskey 1960: 289). Sauceboats, the most distinctive vessels, were often so finished, but some were covered with a fugitive slip, light yellow or gray, or mottled in the firing. Painted patterns were rare.

In EH III, fabrics and shapes were different (Caskey 1965: pls. 51-55). Gray Minyan ware began early—in the second if not in the first phase. A lustrous or semilustrous coating was smeared over surfaces of many pots, more evenly in the first phases and more casually thereafter. Dark painted patterns on a light ground became very common from the start. Coarse jars with prominent knobs on the upper body and other rough jars and jugs with incised patterns (the ware called "Adriatic" by N. Valmin) were popular in the middle and late phases. The sauceboat vanishes from the repertory of shapes, whereas small "ouzo cups" (as we call them) and two-handled tankards (Blegen 1928(2): pl. 13) appear for the first time.

At sites other than Lerna, in the late phase of the EBA, there are shapes that must be fitted into the chronological sequence: the Trojan depas amphikypellon, and one-handled burnished tankards which also clearly have Anatolian affinities (Pl. 3g, h). Lerna gave us a single winged jar (Pl. 3i) that was probably carried across the Aegean from Troy itself (cf. H. Schmidt in Dörpfeld 1902: *Beil.* 36, II). From Lerna comes also a fragmentary marble cup with big swerving handles (Pl. 4a). This shape, like that of the tankards, is closely paralleled at Lefkandi in Euboea (Popham and Sackett 1968: 7, fig, 7, 6), at Ayia Irini in Keos (Pl. 4b), on Mt. Kynthos in Delos, and at Tarsus far away. From Schliemann's Troy, to be sure, came a famous gold cup (Schliemann 1880: 465; Dörpfeld 1902: 353, fig. 284; Matz 1956: pl. 5) which presents us with a discomforting phenomenon: precisely those big handles and double-curved profiles which ought to belong to the late stage, but with them are wing-tipped spouts—not one but two—that are typical of sauceboats.

In our analyses, and our estimates of relationships and dates, shape is obviously an important criterion. But are we led sometimes to rely with too much confidence upon shape alone? Some of the forms noted above as characteristic of EH III can be found again and again, independently created throughout succeeding ages: the ouzo cup; the tankard type with rounded lower body and tall spreading rim; the open cup with fine double-curved profile, exemplified in a well-known bowl that was made in 1768 by Paul Revere (Pl. 4c).

The physical sciences promise ultimately to solve many problems. We have had a preliminary report of radio-chemical studies of ceramics by Michael Attas (1979) of McGill University, Montreal, and the Groupe d'Archéologie Nucléaire at Orsay in France. It is only a beginning, but provides some welcome information.

His testing of sample pottery from contexts of EH II and EH III at Lerna revealed nearly uniform composition, and so confirmed the supposition that most of the pots in both periods were of local manufacture.

Analyses of cruder substances, such as bricks and tiles of EH II, showed erratic values of concentration, their clays being unrefined and having been gathered perhaps from different beds, though surely at no great distance.

The substance of most of the clay sealings, though varied in composition, suggests a local origin; at least one, however, differs conspicuously from the general norms (Attas 1979: 26, no. LER 35 = inv. no. L4.451 (*CMS*, V, no. 112)).

A sherd with light-colored pattern on a dark ground, found in EH III context, is notably different in substance from that of the normal dark-on-light pieces and very probably represents an imported pot (from central Greece? or Crete?).

Obviously, many analyses of this sort, and others, must be made in many regions before a whole picture can emerge. Meanwhile, we continue to apply the traditional methods of archaeological observation and deduction.

EH II and EH III

How great is the difference between EH II and EH III? I must not anticipate the results of the comprehensive studies that are now in progress, but let me state that I still believe, as formerly, that many of the pots do indicate a distinct change in perception, having been made by people of different traditions and with different techniques.

The novelties in EH III at Lerna, and at other sites, are most conspicuous in the wares, shapes and decoration of the smaller bowls and cups; use of the potter's wheel is attested; there are more handles, more patterned surfaces, more variations. But,

to my eye, there is a significant change also in the potter's sense of form. True, this is a personal and subjective observation, based on a single criterion, hard to support pragmatically, and harder still to associate with origins and "race."

Yet consider the apparently simple, but in fact very complex contours, not only in the sauceboats and askoi but even in the plain little bowls and saucers and in the big basins (Pl. 4d), jugs, and storage jars (Müller 1938: pls. 2, 8, 11; Caskey and Caskey 1960: 154, fig. 11, pls. 49, 50; Caskey 1960: 290, fig. 1). Here in EH II there is a tautness and tension that is rarely present in the later EH shapes: a quality that may be seen, if I may choose another distant parallel, in the echinus of an early Classical Doric capital.

The tense and springy form pleases me personally, as you will have guessed. Obviously, it is not to be found in all the plain household vessels of this sort: there are differences in practical requirements; and craftsmen, like artists, vary in talent, skill, and sensitivity. Yet when one sets certain of the earlier shapes besides some typically sagging products of the newcomer, the latter suffer in the comparison.

Carrying the subject further, consider the innovations in Lerna V, the fully established Middle Helladic period. Gray Minyan and some types of coarse wares continue; black Argive Minyan and various kinds of Matt-painted wares begin, as do certain new classes of pots with lustrous paint. Middle Minoan IA and imitations of other MM wares are present for the first time. And in the earliest strata we find small handmade flasks with burnished surfaces, generally dark colored, and elaborate decoration incised or impressed, not stamped (Pl. 4e, f; Caskey 1956: pl. 43b; 1957: 150, pl. 40d, f). They look out of place in a Middle Helladic context. Opinions about their origin have varied widely: that they were imported probably from the Balkans (Bubanj-Hum, Zok); from Romania-Ukraine; from Anatolia; from the Cyclades; from the Saronic region, with Cycladic connections; or that they were, after all, essentially products of the Argolid. Carol Zerner, who is to publish the Fifth Settlement, gives a full survey of the question in her Cincinnati dissertation (1978). Most recently, Mr. Attas has reported in correspondence that, of the three Lerna flasks which he sampled, two show constituents quite unlike those of normal local wares, while the third has a composition slightly nearer the norm though still not conforming. Thus the problem remains, awaiting a decisive solution. Meanwhile, I continue to suppose that the type itself is foreign and that many of the flasks now known were imported (whence, we know not), inspiring possibly some local imitation thereafter.

The sudden appearance of one small distinctive vessel is interesting and may be significant. A more general trend in ceramic design is harder to analyse and to evaluate. Changes in taste and preference occur from time to time, slowly or rapidly,

in all lands and all periods. Here let me call attention to one feature of shape which several of us have observed in the Lerna collection.

The ovoid form with high well-formed shoulder, notable in EH II, which seems to have been lost in most of the repertory of EH III, recurs—essentially, though modified—in an appreciable and increasing number of MH pots. This is not an invariable rule, needless to say.; nor can I give a comprehensive tabulation at present. The fact, however, deserves consideration.

Lawrence Angel recognized the mixture of racial strains in Middle Helladic Lerna. It is admissible to speculate that some innate tendencies of EH II survived, repressed, through EH III, and that the arrival of new people in MH somehow encouraged a recrudescence of old preferences? May the new settlers themselves have brought a different sensibility to form, perhaps long traditional in their own background? The high-shouldered shape is seen early in Lerna V, notably in plain domestic storage jars and coarse wares but ever more apparent in finer pottery. How strong and how influential were the impulses from Crete, or even from Early Bronze Age Troy? I leave the questions unanswered.

And here one further speculation. Some years ago I made another suggestion: that the nature of the land itself may sometimes have played a role. People newly arrived in a new land change and adapt their ways. The land of Greece seems to have what I called a "Hellenizing power" which probably was active long before any people who could properly be called Hellenes had come into existence. The remark was sharply questioned by one attentive reviewer. It was not developed or explained by me at the time, nor, to the best of my knowledge, has it been taken up by others. I was suggesting then, and should like still to suggest, that natural surroundings have their effect upon life not only through physical necessity (ἀνάγκᾳ οὐδὲ θεοὶ μάχονται) but that throughout the ages, intermittently, they may reveal their power by directing manners and aesthetic sensibilities.

This is a delicate and tenuous line of thought. Surely it is not new; but I should like to think that it might be applied to the problems that we are considering. Clearly it is a vast subject, not limited to the Aegean Bronze Age or to the contours of a pot.

Conclusions

What, then, of the question asked, Did the Early Bronze Age end? Speaking of the Aegean, in familiar and well-established terminology, we must, obviously, answer Yes—since Early in that language was followed by Middle. More important, however, are other questions: When? and How?

The problem of absolute chronology I leave to others. Many scholars have tackled it seriously and vigorously; and we know that their conclusions are far from unanimous. Some have applied the comparative method, which has validity, but only if the keystones—say, Egyptian and Mesopotamian dates— are unshakably placed; and they are not. Other methods, aimed at accurate dating through various processes of scientific analysis, promise better results: but we know also that these have not yet reached their goals.

Let me summarize: at Lerna there was a notable break between the periods EH II and EH III: in architecture, in population and burial customs, to some extent in fauna and flora, in types of certain tools and of pottery. I say a notable break, not a total annihilation and supplanting. Undoubtedly some people survived at the site, and with them some vestiges of their former ways of life were preserved.

At Ayia Irini, between the Second and Third Settlements we find some of those changes but not all. Architectural features, relatively simple yet distinctive enough to be recognised, were carried over; and some ceramic forms show continuity, although several others were newly introduced.

The translation from EH III to the Middle Bronze Age at Lerna is far less spectacular than the conflagration that destroyed the House of the Tiles. Some forms continued, some merged gradually; others, however, were innovations.

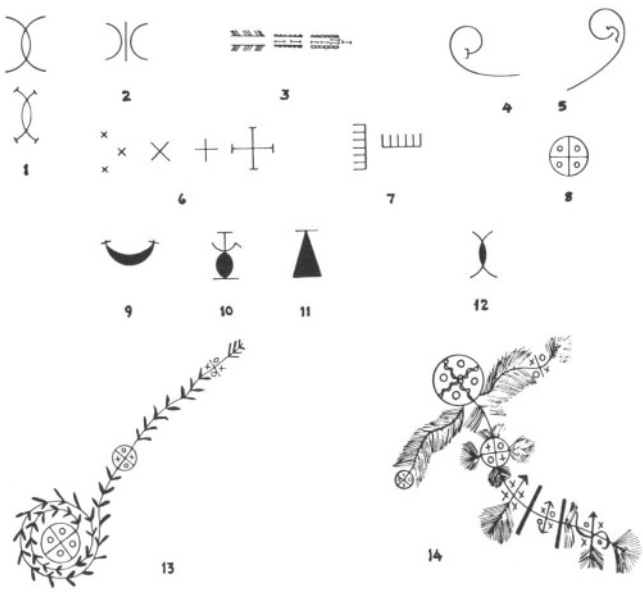
The corresponding division at Ayia Irini was sharper, marked by new orientation of buildings, the addition of fortifications for defense, unprecedented cemeteries, and a quite different repertory of ceramics.

At both sites literacy may have increased with the coming of the Middle Bronze Age, if that may be deduced from the sudden prevalence of potters' marks. Was there a new language?

Much remains to be done before these tesserae can be set to form an understandable mosaic. Within the present limits of our knowledge I look for certain events: the advent of new people (not necessarily hostile invaders) coming quite possibly from Asia Minor, with new knowledge and new energies, to the central and southeastern Greek mainland at the beginning of EH II; the arrival of other vigorous people, again quite probably from the East, some centuries later bringing elements of Anatolian material culture to the Aegean islands, while a related but indepent group, more warlike or meeting stronger resistence, came westward and took many of the EH II settlements by force. and finally, still another immigration or infiltration of eastern and northern peoples who settled rather peacefully with the preceding inhabitants of the eastern mainland, to share in creation of the culture that we call Middle Helladic.

Minoans played an important role throughout these times, rather remote at first, then with intense involvement, and ultimately (in LM III) somewhat withdrawn, though never without effect upon the course of life in the Aegean. We may count on others here present to speak of these facts.

Once I made another of my rash guesses, suggesting that some mainlanders, expelled from their homes by potent newcomers at the end of EC II/EH II, may have taken refuge in Crete and after a time may have contributed to the astounding growth of Minoan civilization in the Palatial period. A friend and astute critic leapt upon that remark as an example of "mainland prejudice." I hope it was not, though freely admitting that youthful enthusiasm over Lerna may have influenced my thought. In any case, I should welcome your comments on that slender theory, as upon any other ideas that I have presented in this rambling exposition.



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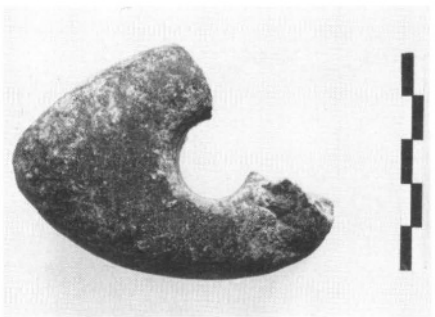

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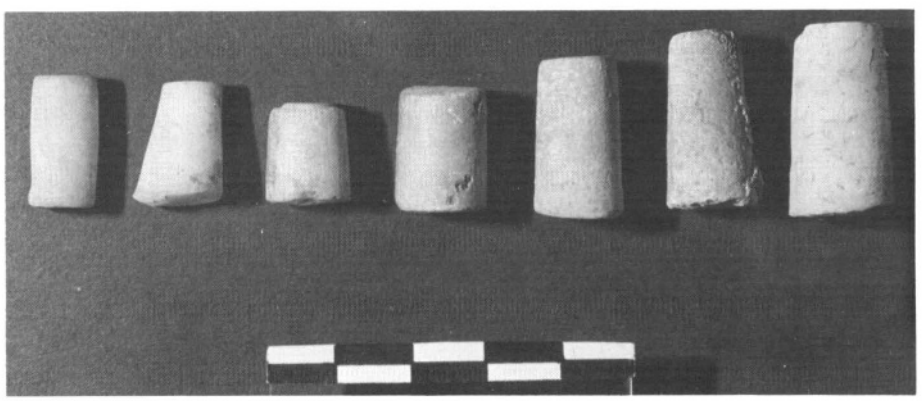
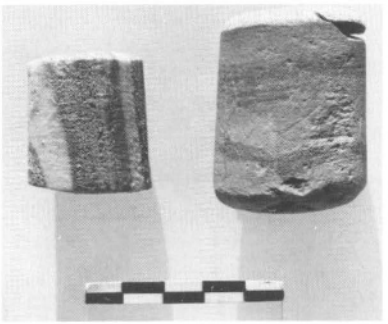


e.

Plate 1: a) Nsibidi ideograms; b) Troy: Feathered spiral; c) Tiryns: EH III patterns; d) Keos: Stone hammeraxe; e) Keos: White marble cores.



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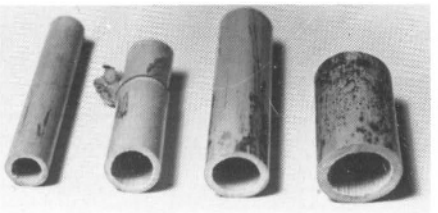

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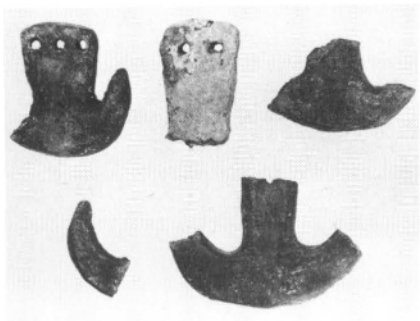
Plate 2

a) Keos: Laminated marble cores; b) Keos: Reeds (bamboo); c) Keos: Stone hourglass pestles, EBA; d) Keos: Stone hourglass pestles, EBA; e) Keos: *Spondylus* shell hourglass pestles, EBA; f) Keos: Hourglass pestle, EBA.



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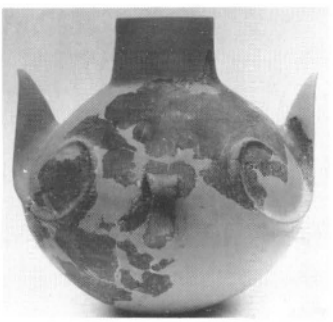
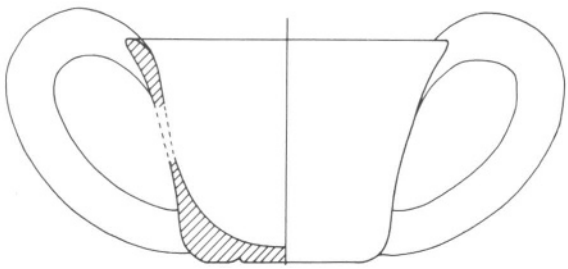
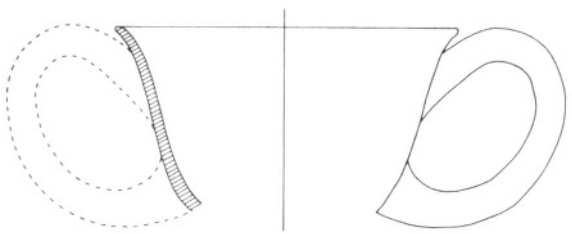


i.

Plate 3

a) Lerna: Clay "anchors", EH III; b) Lerna: Sauceboat, EH II; c) Eutresis: Saucer, EH II; d) Lerna: Patterned jar, EH III; e) Lerna: "Ouzo cup", EH III; f) Lerna: Two-handled bowl, EH III; g) Keos: Depas amphikypellon, late EBA; h) Keos: Tankard, late EBA (h. 0.132); i) Lerna: Trojan wing-jar, EH III context.





b.

c. d.

e. f.

Plate 4

a) Lerna: Marble cup, restored drawing, probably EH III (h. 0.077); b) Keos: Cup, restored drawing, late EBA; c) Paul Revere bowl, silver, (modern replica); d) Lerna: Large basin, EH II; e) Lerna: Handmade flask, Lerna V (h. 0.138); f) Lerna: Handmade flask, Lerna V (h. 0.127).

EVIDENCE FOR INVASIONS IN THE AEGEAN AREA AT THE END OF THE EARLY BRONZE AGE

SINCLAIR HOOD

(Plates 5-10)

The aim of this paper is to suggest that there were two independent and at first unrelated movements of peoples into Greece during the later part of the Early Bronze Age: one on a relatively massive scale from the opposite side of the Aegean, from Anatolia; the other, less well documented in the archaeological record, from Europe to the North.[1] These movements, and especially that stemming from Anatolia, were many-faceted and spread over long periods of time. While the movement from Anatolia seems to have begun before that from the North, the two movements largely overlapped and became intertwined. In the case of the movement from Anatolia the connections with that area are shown by changes in the character of the pottery and by the appearance of a distinctive type of house, apsidal, with one semi-circular or curving end. It will be argued that varieties of apsidal houses were introduced to the Greek mainland from Anatolia rather than from Europe to the North as has often been assumed. This is against the background of a question mark, however, about the relative dating of the early cultural sequences in the Balkans and the Aegean, and in particular about the validity of a chronology based upon calibrated C-14 dates for the Balkans during the relevant period.

Antipathy to the idea of invasion. There seems to be a certain reluctance today to admit that archaeological evidence may reflect population movements, and more

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[1] I am deeply grateful for the invitation to Cincinnati which has given me an opportunity of considering these problems and developing my ideas about them. Dr. Oliver Dickinson and Mr. Roger Howell, whom I consulted, have been most generous of their time and expert knowledge, and I am under a great obligation to them for references, criticism, and valuable advice, which has not been totally neglected. Mr. Howell further lent me his own copy of Wünsche 1977 and an unpublished review of it which he had written. Mr. Jeremy Rutter generously gave me a copy of his important and then forthcoming paper on the Origin of Early Helladic III Ceramics (Rutter 1979) and allowed me to cite it. I am obliged to Miss Jayne Warner for a timely offprint of her recently published account of apsidal and other long houses at Karataş (Warner 1979). Dr. Roger Moorey, Professor J. B. Pritchard, and Mrs. Anita Yannai-James have kindly put me in the way of information about apsidal houses in Palestine and Syria. I am grateful to the Managing Committee of the British School at Athens for permission to describe and illustrate the early D-shaped house found in the School's excavations at Emporio in Chios.

especially invasions accompanied by warfare. But the dawn of recorded history at the beginning of the third millennium opens upon a scene of wars of conquest and movements of peoples. It is indeed possible to form the impression that in early times shifts of population, whether peaceful or warlike, or sometimes perhaps a combination of both, were more frequent than they were to be later. Where civilised states arose in areas like the Nile Valley or eventually (in the Aegean area) in Crete, their powers of organisation and the relatively large numbers of their inhabitants no doubt made conquest by less civilised groups from beyond their frontiers difficult. But even civilised states were exposed to invasion and conquest, as the history of ancient Egypt and Mesopotamia shows.

It might be argued that in our own time this long period marked by shiftings of population combined with invasions and wars of conquest was drawing to a close, and that an awareness of this fact, together with an aversion from the barbarities of the historical and indeed still recent past, tended to inhibit us from acknowledging the material evidence for otherwise unrecorded invasions and wars of conquest in early times. In the Aegean area in particular Nature, in the shape of earthquakes and volcanic eruptions, is conveniently at hand to offer a less unflattering (from the human point of view) even if no less dramatic explanation for destructions, dislocations, and dispersals of peoples.

Archaeological evidence on its own must always be an ambiguous guide, suggesting probabilities and possibilities rather than certainties. But if we see from history that shifts of population, invasions and wars of conquest were, if anything, more frequent in early times than later, and especially frequent in areas beyond the control of civilised states, then we should be inclined perhaps to weigh the balance in favour of an interpretation of the archaeological evidence in such terms when dealing with the Early Bronze Age in the Aegean area.

Roman legend and the foundation stories of Greek colonies suggest some of the infinite variety of forms that movements of peoples and wars of conquest took in early times. At one extreme a whole nation or group of tribes might strive to expel or even exterminate the inhabitants of the land which they wished to occupy. The earliest detailed account of an invasion of this kind is in the Book of Joshua, where God himself orders the Children of Israel to destroy the inhabitants of the Promised Land, although admittedly they fail in this appointed task. While the Bible account cannot be regarded as strictly historical, it describes attitudes and conduct that may have been all too general in the case of relatively uncivilised peoples invading a more civilised area in those days. At the other extreme, small groups of newcomers may have persuaded rather than compelled existing inhabitants to accept them as rulers, or as equals sharing the land, or even as subjects.

Invasion hypotheses. The number and the complexity of the various movements of peoples southwards into the Greek peninsula in historic times, and especially during the period of a thousand years between the break-up of the Roman Empire around 500 AD and the Turkish conquest in the fifteenth century, have recently been described by Hammond (1976). A study of the small-scale migrations or infiltrations by nomadic and semi-nomadic peoples such as the Vlachs during this long period may have a special relevance for the problem of movements into Greece from Europe to the North at the end of the Early Bronze Age as Hammond emphasizes.

It is widely believed that various features which appear in the archaeological record of the Greek mainland for the first time at the end of the Early Bronze Age reflect one or more episodes of invasion or infiltration by newcomers from Europe to the North. An all-embracing version of this view has been expounded by Gimbutas (1973) and by Hammond, who argue that successive groups under leaders originating from the Kurgan (Yamnaya or Pit-grave) culture of South Russia moved into Greece, reaching as far South as Lerna in the Peloponnese before the end of the Late Neolithic there, and eventually introducing the custom of tumulus burial, apsidal houses, stone shaft-hole hammers and hammer-axes, and certain types of pottery, notably corded ware.

There does indeed seem to be archaeological evidence for infiltration of some kind into Greece from the North towards the end of the Early and the beginning of the Middle Bronze Age. But this evidence is still rather sparse, and some of it is ambiguous. Moreover, as I shall attempt to demonstrate, any infiltration which there may have been from the North must be seen against the background of other movements of people reaching the mainland of Greece from the opposite side of the Aegean, from western Anatolia. The movements from Anatolia seem to have begun earlier, and were on a very much larger scale, than any infiltrations from the North.

Certain traits, notably the practice of burial in tumuli, the use of hollow-based arrow-heads (Pl. 10a) and of arrow-shaft smoothers, together with some rare kinds of exotic pottery including corded ware, can reasonably be interpreted as reflecting the presence of Northerners in Greece. But the basic innovations in pottery in Greece at the end of the Early Bronze Age point to Anatolia, and it will be suggested that the apsidal house, and also probably, in the first instance at any rate, the fashion for stone shaft-hole tools and weapons, reached the Greek mainland from Anatolia and not from Europe to the North.

These movements of people into Greece, whether from Europe to the North or from Anatolia, were evidently complex, involving a number of different groups, and were spread over a considerable period of time. There is reason to suppose that they overlapped to some extent; but at the key site of Lerna in the northeastern Pelopon-

nese the Early Helladic III period was apparently inaugurated by people of the Anatolian movement, while possible evidence for Northerners there is only attested at the beginning of Middle Helladic a century and a half or more later.

Anatolian movement. It has become increasingly clear during the past ten years that Anatolian features, especially in pottery, began to make their appearance in the western part of the Aegean towards the end of the Early Bronze Age there. French (1972: 23) in particular has suggested that much more came across to Greece from Anatolia in Early Helladic III than has been suspected. It seems difficult to interpret this appearance of Anatolian features as simply reflecting a spread of fashions, especially if the introduction of the apsidal house to the southern part of the Greek mainland is brought into connection with it. Changes in burial custom may also point to a movement of people from Anatolia, although the evidence bearing on these is comparatively slight at present.

The Anatolian movement is divisible into three main groups, which appear to have first reached the Greek mainland one after the other. The initial settlements of each group were established in different areas, beginning in the North and ending in the South; but there was eventually some degree of overlap, settlers of one group replacing or merging with those of another. (1) The first (Macedonian) group of settlers established themselves in Macedonia inaugurating the local Early Bronze Age, which is characterised by the sudden appearance of pottery of vaguely Troy I type. (2) Settlements assignable to the second (Lefkandi I) group are found in some of the Cycladic islands and in Euboea, as well as on the mainland at Pefkakia opposite Volos. These settlements appear to have been founded before the end of the Early Helladic II period as defined at Lerna. Wheelmade and other pottery of the type at home at Troy from early in Troy II onwards is characteristic of this group. (3) The third and latest (Early Helladic III) group of possible Anatolian settlers inaugurated the Early Helladic III period in central and southern Greece.

The character of the evidence available for each of these three groups varies, and any conclusions based upon that evidence must also consequently vary in degree of probability. In the case of the first (Macedonian) and the third (Early Helladic III) groups, indications that we are faced with invasion or immigration and the settlement of new people are fairly strong; but in the case of the second (Lefkandi I) group, to judge from the limited amount of evidence so far available from publications, they are perhaps less so. This second (Lefkandi I) group also appears to have been less uniform, with greater regional differences, not only in the type of pottery used, but also in such aspects of life as housing and burial practices.

At the same time certain features appear to be common to all three groups or are shared by two of the three. Thus wheelmade pottery of the type at home in western

Anatolia from the time of Troy II onwards is a mark of all three groups, although its occurence on sites of the first (Macedonian) group is sporadic. There is a suggestion, even if only a faint one, of possible shared traditions in burial customs, and in the types of houses built, although the evidence bearing on these is still very imperfect. Distinctive clay "anchor ornaments" (pl. 9d) are characteristic of the first (Macedonian) and third (Early Helladic III) groups, but do not seem to be recorded from sites of the second (Lefkandi I) group. Stone tools with neat cylindrical perforations for wooden shafts are attested for the first time in Macedonia with the beginning of the Early Bronze Age there, and in southern Greece first it would seem in Early Helladic III.

(1) *Macedonian Early Bronze Age group*. It has been more or less dogmatic since the time of Heurtley (1939) that the Macedonian Early Bronze Age was inaugurated by settlers from somewhere in northwestern Anatolia. This view has recently been restated by Hanschmann and Milojčić (1976), who have republished much of Heurtley's material and have once again drawn attention to the way in which the evidence of the pottery suggests that people who had first settled on the coast in the region of Kritsana spread northwards and westwards, mixing to some extent with the previous inhabitants. Eventually some groups appear to have moved down into Thessaly, where the pottery of the local Early Bronze Age, which began after that of Macedonia, has many affinities with that of Macedonia and may derive from it. This was the view of Heurtley (1939: 111, 129), and the evidence of the pottery appears to be in harmony with it; but it must be noted that Hanschmann and Milojčić (1976: 224) stress influences from the South of Greece in the formation of the Thessalian Early Bronze Age.

The Early Bronze Age pottery of Macedonia is distinctly Anatolian in character, and is reminiscent, although only in a general way, of that found in Troy I. But even the earliest stages of the Macedonian Early Bronze Age may overlap with Troy II, when wheelmade pottery first appears at Troy. At Kritsana, which appears to be the earliest known settlement of the Macedonian Early Bronze Age, wheelmade pottery is first attested in the third of the six Early Bronze Age occupation levels (Heurtley 1939: 80, n. 1; Hanschmann and Milojčić 1976: 199). Wheelmade pottery seems to have been somewhat rare at Kritsana, however, and all of it may have been imported. It is not a feature of Early Bronze Age sites explored in the hinterland of Macedonia, apart from Servia, where Heurtley noted two possibly wheelmade vases (Heurtley 1939: 180; cf. Hanschmann and Milojčić 1976: 211). Wheelmade pottery does not seem to occur on inland sites in Thessaly alongside the pottery of standard Thessalian Early Bronze Age type related to that of Macedonia.

There appears to be virtually no information available for the types of houses or

burial customs that were current in the Macedonian Early Bronze Age as known from the central and western parts of Macedonia. But apsidal houses were built at Sitagroi in eastern Macedonia during the local Early Bronze Age which seems to be related to that of the Centre and West. An apsidal house (P) at Rakhmani in Thessaly is assigned to the Early Bronze Age there.

(2) *Lefkandi I group*. The evidence for our second group comes from a scatter of sites in the northern Cyclades (e.g. Kastri at Chalandriani on Syros, and Ayia Irini on Keos), and in Euboea (notably Lefkandi and Manika), as well as from the important site of Pefkakia on the South side of the Gulf of Volos. While the pottery of the first group (1) is reminiscent of that of Troy I, that of the second (2) has distinct affinities with that of Troy II-III and EB III in Cilicia, with wheelmade wares much in evidence. Considerable regional variations are apparent within this group, and these could reflect the presence of people deriving from different parts of Anatolia. There does not seem to be any uniformity in the types of houses built, and there was probably a corresponding lack of uniformity in burial customs. Thus at Kastri at Chalandriani on Syros the houses shared party walls and were packed close together behind fortifications (Pl. 5g), while at Pefkakia (pl. 7b) free-standing long houses, one of them with an apsidal end, were akin to those characteristic of the third (Early Helladic III) group.

It may seem more doubtful in the case of this second group than it does in the case of the first (Macedonian) and third (Early Helladic III) whether in fact an invasion or immigration of any kind is involved. But at Kastri at Chalandriani the settlement of this group occupied a different site from what appears to have been a previous Early Cycladic II settlement across the gorge from it (Tsountas 1899: 78). It is usually assumed that the great Early Bronze Age cemetery at Chalandriani continued in use during the life of the fortified settlement on Kastri. But pottery of the type found in the Kastri settlement is hardly attested from the cemetery (Bossert 1967). This difference in the pottery had already struck the original excavator, Tsountas, who explained it by suggesting that the vases from the tombs were luxury articles or made especially for funerary use (Tsountas 1899: 127). Moreover, as Professor Caskey (1964: 64) has noted, it is relatively difficult to reach the Kastri settlement from the cemetery area. It might therefore be worth considering whether the graves that belonged to the Kastri settlement were not in its neighbourhood and are still awaiting discovery on the opposite side of the gorge from the main cemetery.

The settlement of this group (2) at Lefkandi was evidently on a virgin site, although a previous Early Helladic II settlement may have existed somewhere near at hand. At Pefkakia the settlement of this group was on the site of that previously occupied at the beginning of the Early Bronze Age, but the earlier defences were

abandoned, and the free-standing houses may have been new types introduced here for the first time, although this is by no means certain from the evidence available. The fact that the houses were free-standing with gaps between them suggests, although it does not prove, that the settlement of group (2) at Pefkakia was unfortified, as the Early Helladic III settlements of southern Greece with similar free-standing houses appear to have been.

(3) *Early Helladic III group*. The chief excavated sites of this group are Lerna and Tiryns in the North East Peloponnese, and Eutresis, Orchomenos and Ayia Marina in central Greece. The Kolonna settlement on Aigina was also clearly important in Early Helladic III. The pottery of group (3) appears to be relatively homogeneous in character, in spite of regional and local variations; but it is not so obviously Anatolian as that of groups (1) and (2). At the same time much Early Helladic III pottery is wheelmade, like that of group (2), and the fashion for monochrome burnished surfaces which is a characteristic feature of it also points to Anatolia.

There seems to be a considerable degree of uniformity in the architecture of group (3), with free-standing long houses which often have apsidal ends. The appearance of these free-standing long houses at Lerna in association with radical changes in the character of the pottery strongly suggests that the Early Helladic III period there was inaugurated by invaders or immigrants, as Professor Caskey (1966) and others have claimed.

Distribution of settlements. The areas which were eventually occupied by the settlements of these three groups were more or less limited in extent. (1) The Macedonian group spread from what seems to have been an original centre on the East side of the Gulf of Salonika round Kritsana northwards and westwards and eventually into Thessaly. (2) The settlements of the second (Lefkandi I) group appear to be largely confined to some of the northern islands of the Cyclades, and to Euboea. Doumas (1977: 22-23, 25) includes the Mt. Kynthos settlement on Delos and sites on Naxos and Siphnos in this group. But the material from the islet of Christiana South West of Thera in the southern Cyclades assigned by him to it does not appear to be typical according to Rutter (1979: 20, n. 5). The settlement at Pefkakia is so far unique on the Greek mainland, but other sites of group (2) may await discovery on the coast further South opposite Euboea and perhaps inland from there.

(3) The third (Early Helladic III) group is rather densely represented in the northeastern Peloponnese, in the Argolid, and also in central Greece, and there was an important Early Helladic III settlement on the island of Aigina. Eventually at any rate, the fashion for Early Helladic III type pottery spread to Arcadia (Asea) and as far West as Elis (Olympia) and Ithaca (Pelikata); but there is no evidence as yet that the distinctive apsidal house was associated with it in those areas. Apsidal

houses at Malthi in northern Messenia have been described as Early Helladic, but even the earliest occupation levels there appear to date from Middle Helladic times (Howell 1975: 111). Typical Early Helladic III pottery has not been found at Malthi or anywhere else in Messenia or Laconia (Rutter 1979: 13, 15). A similar situation exists in Attica, where Early Helladic III pottery hardly seems to be attested.

Apsidal houses. The long house with an apse at one end is typical of our Early Helladic III group (3). It may also have been among the types of house current in our second (Lefkandi I) group, to judge from the evidence from Pefkakia, combined with that of a more disputable character from the Mt. Kynthos settlement on Delos and that from Kastri at Chalandriani considered below. There is also room for the suspicion that apsidal houses were standard in the Early Bronze Age of Macedonia (1).

It must be remembered, however, that not all apsidal houses of Bronze Age or earlier date in the Aegean area were long. A simple one-room type of short apsidal or D-shaped house, which is attested in the Bothros horizon assigned to Early Helladic III at Orchomenos (Pl. 6d) and later in Middle Helladic times at Malthi in Messenia (Pl. 6e) and at Krisa near Delphi (Pl. 6f), has earlier analogues in the Cyclades (Mt. Kynthos on Delos, Pl. 6c) and at Emporio on Chios (Pl. 5a).

Once established in Greece the apsidal house enjoyed a long history. Such houses are found in most parts of the Greek mainland in the Middle Helladic period, and they are attested in the Late Bronze Age in some areas of the Mycenaean world. At the beginning of the Early Iron Age there was a revival of the fashion for apsidal buildings in many parts of Greece, whether as a result of their survival in certain regions, or owing to the intrusion of new groups of people who were used to living in them into the Mycenaean world from the North at that time. In this connection it is interesting to note that apsidal buildings also make their appearance in Italy as far South as Sicily at the beginning of the Early Iron Age there (Allen 1977: 482-84, 490-94).

Long houses of one kind or another are attested relatively early in the Near East (e.g. Hrouda 1970), and such houses were being constructed in Greece by the later part of the Neolithic there (Hammond 1976: 91). Similarly in Bulgaria there appears to be a change from houses of square ground plan to ones of rectangular shape in the horizon of Karanovo IV (Georgiev 1961: *Beil.* B (at end)). But the house with an apsidal end has a very much more limited distribution in time and space. Apsidal houses only seem to occur in three other areas besides Greece before the beginning of the Iron Age: (1) Palestine and Syria, notably in the horizon which immediately precedes or inaugurates the Early Bronze Age there (de Miroschedji 1971: 43-44, 97, 124; cf. Yassine 1978; Thompson 1969; de Contenson 1961); (2) western

Anatolia and the offshore islands like Chios and Lemnos from a period corresponding to the Kum Tepe I A phase in the Troad until at least the time of Troy I; and (3) the Balkans and other parts of Europe further North from the end of the Late Neolithic onwards.

Whether there is any connection between the apsidal houses of Palestine and Syria and those of other regions is not our immediate concern. A well preserved apsidal house at the great coastal site of Byblos in Syria is assigned to the Early Neolithic (Dunand 1973: 24-25, fig. 9, pls. 9, 2; 16, 3). If this date is correct it would be much older than any of the apsidal houses of Anatolia and surrounding regions including the Balkans. The problem before us, however, is whether the Greek apsidal houses, which in the North East Peloponnese at any rate seem to appear suddenly after the end of Early Helladic II along with other elements of Early Helladic III culture, ultimately derive, as has often been assumed, from Europe to the North, or from Anatolia.

Various suggestions have been made in the past about the origin of the distinctive apsidal plan (Robertson 1943: 25-26). It has been regarded as a stage in evolution from a circular hut to a rectangular house (e.g. Bülle 1907: 47-51; Dinsmoor 1950: 4-8), or as a development from a cave with a screen across the front of it (Leroux 1913: 17-22, 26-28). In the case of the apsidal houses of the Greek mainland in the Middle Bronze Age, persistent efforts have been made to find prototypes for them in Europe to the North or North East. Thus Boëthius (1921: 173, pl. 6, 4) drew attention to very primitive apsidal structures assigned to the Neolithic at Klein Meinsdorf in Schleswig-Holstein as possible ancestors of the apsidal houses of Greece. But these early apsidal structures in Northern Europe appear to be versions of primitive shelters, and there do not seem to be any intermediary links connecting them with the large timber-built apsidal houses eventually found in the Balkans and Central Europe.

It has also been suggested that the apsidal house is merely a development of the kind of flimsy circular hut built by nomads, and reflects the type of dwelling used by pastoral semi-nomadic people who introduced it to Greece (e.g. Dinsmoor 1950: 4-7; Caskey 1971: 788; Hammond 1976: 119). But there does not seem to be any evidence to suggest that the pastoral and semi-nomadic people of the Kurgan (Yamnaya or Pit-grave) culture of South Russia built apsidal huts or houses, and when apsidal houses appear in the Balkans and Central Europe they are relatively large and massive timber structures which hardly reflect a nomadic background.

Apsidal houses do not seem to be much in evidence in the southern part of the Balkans before the end of the local Neolithic. None is noted by Tringham (1971) in her comprehensive survey of the early cultures in that area. But large apsidal houses

built on a framework of wooden posts without the use of stone foundations in Hungary and Czechoslovakia have been dated to the horizon of the Lengyel culture, which appears to be contemporary with later Vinča in Yugoslavia and with Gumelniţa in Romania and Bulgaria. The rounded end of such an apsidal house at Zengovarkony in Hungary is thought to have been a porch or vestibule (*Vorhalle*) (Dombay 1960: 57, fig. 17; Müller-Karpe 1968: 480, pl. 188M). But other apsidal houses at Veszprém in Hungary and at Bylany in Czechoslovakia assigned to late Lengyel seem orthodox in plan with entrances in the narrow ends opposite the apses (Raczky 1974; Soudsky 1966: 71, fig. 19; Müller-Karpe 1968: 482, pl. 194H) (Pl. 7e). The imposing group of apsidal buildings, chieftain's house and cookhouse-cum-workshop, at Vučedol in northern Yugoslavia belongs to the horizon of the Baden culture (R. R. Schmidt 1944). Later in the Bronze Age apsidal houses are found at least as far West as Belgium and Holland (Coles and Harding 1979: 284-86, figs. 97-98).

Apsidal houses do not seem to be attested in Bulgaria before the Early Bronze Age which succeeds Gumelniţa there. Then, however, they appear to be very much at home and are reported on a number of sites like Ezero and Karanovo (Coles and Harding 1979: 134; Georgiev 1961: *Beil.* B (at end) (Karanovo); 1967: 120, 130, fig. 26 (Ezero); Piggott 1965: 112, pl. 12b (Karanovo)). A wooden apsidal house like those found in Bulgaria and other parts of Europe to the North has been excavated in a corresponding Early Bronze Age horizon (Phase Va) at Sitagroi in eastern Macedonia (Renfrew 1972: 119-20) (Pl. 7f). It is interesting that the pottery of this Early Bronze Age horizon in Bulgaria and eastern Macedonia has close affinities with that of northwestern Anatolia in the period of Troy I-II on the one hand, and with that of the central Macedonian Early Bronze Age (Kritsana) on the other. The appearance of apsidal houses for the first time in Bulgaria in this horizon might therefore be taken as a symptom of Anatolian influence also reflected in the pottery, if it could be proved that houses of this type were at home in Anatolia at an earlier date.

The apsidal houses that have been excavated in the Balkans and Central Europe were built with a framework of wooden posts without any use of stone foundations. But virtually all the known examples of such houses in Greece, Turkey and Palestine have foundations or socles of stone. There are some exceptions, however, in Greece. Thus the remains of an apsidal hut (ἀψιδωτῆς καλύβης) built without stone foundations were reported by Theochares (1954(1): 105) at a spot about 500 metres West of the main settlement at Askitario in Attica. No evidence for the date of this apsidal hut is given, but it is implied that it was Early Helladic. Better known is an apsidal house built without the use of stone at the beginning of the Early Helladic III period

at Lerna. This was the earliest of a series of apsidal houses which may have been occupied by the local chieftains or rulers of the Early Helladic III settlement there (Caskey 1966). The apsidal houses which succeeded it were built with stone socles or foundations, and the fact that only the earliest of them had a framework of wooden posts might seem to point to a northern origin, since the apsidal houses found in Europe to the North are similar.

There appears to be no reason, however, why apsidal houses built without the use of stone should not have also existed in early times in Anatolia. Wooden buildings, small houses or barns, have been identified at Karataş in southwestern Turkey in an Early Bronze Age horizon in which long houses marked by rather flimsy stone foundations, some of them with apsidal ends, also occurred (Mellink 1968: 248-53). In her recent study of the Karataş long houses Warner (1979) has drawn attention to the fact that wattle-and-daub was the predominant method used in constructing their walls, and has noted that the Early Helladic III wooden house at Lerna similarly had walls of wattle-and-daub. But wattle-and-daub construction was also at home in the Balkans in the local Early Bronze Age and before it (Warner 1979: 144-45).

There are strong indications that wooden houses, or houses with walls of wattle-and-daub, built with vertical posts sunk in the ground but without stone foundations, were very much at home in early times in the northern parts of Anatolia, that is to say, in the parts in closest proximity to the Balkans. Thus at Fikirtepe on the Sea of Marmara the houses were built entirely of wood and incorporated vertical wooden posts (Yakar 1975: 133-34). These houses at Fikirtepe were associated with a culture dating from before the time of Troy I and assigned by some to the horizon of the Anatolian Late Neolithic or Early Chalcolithic, but more likely perhaps Late Chalcolithic. At Ikiztepe on the Black Sea coast East of Sinope the earliest houses of the beginning of the Early Bronze Age or earlier were of wattle-and-daub on post frames (Yakar 1975: 135; cf. Alkim 1979: 201). At Demirci Hüyük the Early Bronze Age buildings similarly appear to have been mostly of wood with posts (Korfmann 1979: 193).

There does not seem to be much information available about the plans of these early wooden buildings of northern and northwestern Anatolia. No apses have been reported in connection with them, but an apsed building of Middle Bronze Age date has been noted at Demircihüyük (Korfmann 1979: 194).

The earliest apsidal houses in the region of western Anatolia are certainly much older than any that have yet come to light on the Greek mainland, and even with a chronology for the Balkans and Central Europe based upon calibrated C-14 dates it seems possible that some of them are older than any known in Europe to the North of Greece.

A. Sampson kindly allows me to mention his discovery of evidence for the possible existence of apsidal houses in the Dodecanese back in the local Neolithic. Apsidal houses may therefore prove to have been current in this Eastern part of the Aegean area even before the horizon of Period VIII at Emporio in Chios, in a level of which the D-shaped room of Pl. 5a was exposed. Emporio VIII evidently belonged to the pattern burnish horizon of Beşik Tepe (Kum Tepe IA) which preceded that of Kum Tepe IB before Troy I in the Troad. There was no evidence for the existence of a rectangular chamber in front of this D-shaped room in Emporio VIII, and the slope of the ground here made it rather unlikely that one had ever existed. A doorway with the threshold stone in place led into the room through the curving side. It is therefore possible that this was a small single-room D-shaped house like some later houses of comparable plan in the Cyclades and on the Greek mainland. These later D-shaped houses occur in the same horizon as apsidal houses of the standard long type and appear to be related to them.

No complete house plans were recovered from the succeeding Kum Tepe IB horizon (Periods VII and VI) at Emporio. But the short stretches of curving wall exposed in soundings made into levels of the Black Period (I) at Poliochni on Lemnos, contemporary with Emporio VII-VI and with Kum Tepe IB in the Troad, could have belonged to apsidal houses (Mellaart 1978: 86; Warner 1979: 138; e.g. Bernabò-Brea 1964: 53-57, figs. 25-28; 86-96, figs. 45-55) (Pl. 5b). The only house of which the plan was recovered in the earliest level at Troy (Troy Ia) was apsidal of the standard long-house type with an apse at one short end (Blegen *et al*, 1950: 82, 86, fig. 425: House 103) (Pl. 5c). It is interesting that there was evidence for the existence of a door in the side of the apse in the same position as the door into the D-shaped room of Emporio VIII (Pl. 5a). This apsidal house at Troy was free-standing, separated by a space of open ground from its neighbours.

In succeeding levels of Troy I, as in contemporary Thermi on Lesbos (Pl. 5d) and in Emporio V-IV, long houses were usual, but they had no apses. One reason for the lack of apses may be that in major centres like these the houses came to be packed close together, sharing main outside walls, instead of standing free with open ground between them as they had in the earlier levels of Troy I. This more compact system of town-planning with houses sharing party walls could reflect an increasing population confined behind fortifications so that space was at a premium. Houses of strictly rectangular plan would be easier to combine together in this manner. The adoption of party walls may also have involved a change in the system of roofing, from pitched roofs of the kind which free-standing apsidal houses seem to invite, to flat roofs, long at home in many parts of Anatolia.

Nevertheless at Poliochni in the Blue Period (II), which may be equated with the

early part of Troy I, clear vestiges of an apsidal building tradition survive (Bernabò-Brea 1964: atlas pl. 4: areas 206, 211, 212) (Pl. 5e, f). Similar vestiges have been recognised in the fortified settlement of Kastri at Chalandriani on Syros which apparently belonged to people of our second (Lefkandi I) group (Leroux 1913: 7; Bossert 1967: 59; Caskey 1971: 796) (Pl. 5g).

There may have been a considerable gap in time between the free-standing apsidal house of Troy Ia and those of Early Helladic III in southern and central Greece. A gap of some centuries is still left with a date for the end of Troy II around 2000 B.C. or later as advocated by Maxwell-Hyslop (1970; 1971: 57-60) and myself among others on the basis of the latest jewelry in a Mesopotamian style from the great Treasure A lost at the time that Troy II was destroyed. But, as we have already noted, at Karataş in southwestern Anatolia Miss Mellink (1968) has reported the discovery of a horizon which appears to be contemporary with the early part of Troy I with apsidal houses with stone foundations alongside small free-standing wooden houses or barns, while an apsidal structure of Middle Bronze Age date has been recognised at Demirci Hüyük in the North West of Anatolia, a region where a tradition of building in wood appears to have persisted from very early times. It therefore seems a real possibility that a custom of building free-standing apsidal houses, including wooden ones, survived throughout the earlier part of the Bronze Age in some parts of the hinterland of Anatolia and was brought thence to the Cyclades and ultimately at the end of Early Helladic II to the Greek mainland. The idea of building apsidal houses may also have spread from Anatolia to the Balkans, although perhaps at an earlier date.

There is evidence to suggest that the fashion for building apsidal houses became established in some of the northern islands of the Cyclades and in Euboea before the end of early Helladic II in mainland Greece. What appears to have been part of an apsidal house excavated by Sampson at Mourteri on the East coast of Euboea was evidently associated with Early Helladic II pottery (Sampson 1978: 245-54, figs. 3-5, and 257-58). Doumas (1972: 170) has emphasised that houses with curving walls seem to appear in the Cyclades for the first time in an advanced phase of Keros-Syros (Early Cycladic II). Some of the examples to which he draws attention, notably at two sites, Pyrgos and Paroikia on Paros (Pl. 6a, b, and in the settlement on Mt. Kynthos on Delos (Pl. 6c), are definitely apsidal, and others which he illustrates (e.g. at Panormos and Korfi t' Aroniou on Naxos) look as if they might have been (Doumas 1972: 157, fig. 8, 161, fig. 14). The one at Korfi t' Aroniou, which I have seen, is indeed rather comparable in size and shape with the D-shaped room of Emporio VIII.

Lawrence, who drew attention to the apsidal houses of the Early Bronze Age in

the Cyclades, suggested that their shapes might have been deliberately reproduced in some Early Cycladic tombs, including built ones in the Chalandriani cemetery on Syros, and ones cut in the rock at Manika in Euboea (Lawrence 1973: 12-13). None of the Chalandriani tombs, however, seems to reproduce an apsidal plan in the same deliberate way that Protogeometric stone-built tombs near Karpofora in Messenia do (Yialouris 1960: 108); the Karpofora tombs are neatly matched by contemporary Dark Age apsidal houses in the neighbouring settlement of Nichoria. It also seems rather doubtful from the published plans whether any of the Manika tombs were consciously imitating apsidal houses (Papavasileiou 1910). The clay vases from the tombs at Manika, however, are distinctly Anatolian in character and assignable to our second (Lefkandi I) group, while French (1966) has reported Lefkandi I type pottery as well as Early Helladic III from the settlement there.

The free-standing apsidal houses at Pyrgos and Paroikia on Paros in each case belonged to a horizon which succeeded one with rectangular-roomed houses. This suggests the possibility that the apsidal type of house was intrusive on Paros. The house at Paroikia (Rubensohn 1917: 8-11) (Pl. 6a) was clearly later than a rectangular-roomed one which its walls enclosed, and it may have been of Early or Middle Bronze Age date like the rest of the prehistoric settlement there. But important pieces of Geometric pottery were found in this area at the foot of the Acropolis (Rubensohn 1917: 73), and the apsidal house might conceivably have belonged to that period, since buildings of apsidal plan reappear in many parts of the Greek world in the Early Iron Age (Snodgrass 1971: 369; Sinos 1971: 109-10); there were apsidal houses for instance in the Ionian city of Old Smyrna in Geometric and into Archaic times (Sinos 1971: 109). The apsidal house uncovered by Tsountas (1898: 168-75) at Pyrgos, however, appears to date from a time when pottery assignable to Early Cycladic II was in use (Doumas 1972: 164-65) (Pl. 6b).

Some apsidal structures were noted in the prehistoric settlement on top of Mt. Kynthos on Delos (Plassart 1928: 11-18, 21-23). These may also date from Early Cycladic II, that is, from before the beginning of Early Helladic III on the Greek mainland. Pottery published from the settlement includes sauceboats (Plassart 1928: 43-44) akin to those of Early Helladic II, and there appear to be links between some of the pottery and that from levels of Period IV at Emporio assignable to the time of Troy I (e.g. the large jar, Plassart 1928: 40, fig. 38). There are also links, however, with the Early Helladic III pottery of the Greek mainland, and with pottery typical of some sites of our second (Lefkandi I) group (Rutter 1979; MacGillivray 1979).

One at least of the apsidal structures on Mt. Kynthos, Cabane Psi, was relatively well preserved and appears to have been a simple one-room type of house (Plassart

1928: 22-23) (Pl. 6c). A distinctive feature of Cabane Psi was the comparative thickness of the straight wall forming the base of the apse. This thickening was evidently deliberate, and it looks as if the wall had been intended to support the thrust of a roof in the form of some kind of half-dome. Sinos (1971: 36-37) has drawn attention to comparable one-room apse-houses in the Bothros horizon at Orchomenos in Boeotia assignable to Early Helladic III (Bülle 1907: 34-36, fig. 9, Typus C) (Pl. 6d).

Another site with a number of what appear to be relatively small one-room apse-houses is Malthi in Messenia (Valmin 1938: 25-30) (Pl. 6e). The excavator, Valmin, assigned these to the earliest horizon of occupation there, and argued that they were succeeded by a horizon with rectangular-roomed houses. But the evidence for this succession appears to be ambiguous, and it might be possible to invert it. In any case the earliest occupation at Malthi does not seem to date from before the beginning of the Middle Helladic period (Howell 1975: 111). In later Middle Helladic times long apsidal houses of conventional plan were built on the site. Part of what seems to have been a small one-roomed apsidal house of the Middle Helladic period resembling those of Malthi was excavated at Krisa below Delphi (Jannoray and van Effenterre 1937: 306, fig. 7, pl. 25; Sinos 1971: 81) (Pl. 6f).

These relatively small one-roomed apsidal houses are important, because comparable houses do not appear to be attested in Europe to the North. Some ambiguity could surround the derivation of the standard type of long apsidal house from Anatolia, in view of the wooden example from the earliest level of Early Helladic III at Lerna, and the occurrence of similar large wooden apsidal houses—some of them, if a chronology based upon calibrated C-14 dates is accepted, much earlier in date—in Europe to the North. But in the case of the small one-room type of apsidal house the evidence points more unambiguously to a derivation from Anatolia, with the discovery of what appears to have been a prototype for it in an early horizon at Emporio in Chios (Pl. 5a).

Free-standing apsidal houses seem to be characteristic of our third (Early Helladic III) group. Two long houses were exposed in the horizon of our second (Lefkandi I) group at Pefkakia (Pl. 7b); one of them was rectangular in plan, and the excavator compared it with the megara of Troy II (Milojčić 1974: 45); but the other was apsidal. It is clear, however, that not all settlements assignable to our second (Lefkandi I) group had free-standing long houses. Thus in Kastri at Chalandriani on Syros (Pl. 5g) the houses, close-packed behind fortifications and sharing party walls, were not strictly apsidal in plan, although with a good will some vestiges of an apsidal tradition can be traced in them (Leroux 1913: 7; Bossert 1967: 59). Similar vestiges, as we have seen, can with less ambiguity be recognised in the horizon of the Blue

Period (corresponding to early Troy I) at Poliochni on Lemnos (Pl. 5e, f). Poliochni and Kastri were both fortified, however, and the close-packed houses sharing party walls by implication had flat roofs. But the settlement at Pefkakia during the period of the group (2) occupation may have been an open one without defence walls and consequently with space for free-standing houses.

At the same time there is evidence to suggest that apsidal long houses were already at home in parts of Thessaly before the period of the group (2) occupation at Pefkakia, which is assigned to the middle phase of the local Early Bronze Age. Thus at Rakhmani further North in Thessaly a free-standing apsidal house (Q) (Wace and Thompson 1912: 37-40) (Pl. 7c) belonged to the 'Chalcolithic' Rakhmani culture, which at Pefkakia immediately preceded the Early Bronze Age there. Finds from House Q included vases of classic Rakhmani Crusted ware, which Hanschmann and Milojčić (1976: 146, 152-53) have assigned to an early phase of the Rakhmani culture. Among other objects from House Q were a couple of stone hammers with shaft-hole perforations (Wace and Thompson 1912: 43, fig. 28o).

Stone tools with neat cylindrical shaft-holes bored through them were at home in Anatolia from relatively early times; but they do not appear to be attested in Macedonia before the beginning of the local Early Bronze Age (cf. Renfrew 1972: 120), nor in South Greece before Early Helladic III. Stone shaft-hole tools were in use in the Gumelniţa horizon in the Balkans, however, and also in that of the Lengyel culture further North, and even it would seem in that of the Linear pottery at Bylany in Czechoslovakia (Soudsky 1966: pl. 14, including a bore-core). These contexts are almost certainly earlier, and with a chronology based upon calibrated C-14 dates considerably earlier, than the beginning of the Early Bronze Age in Macedonia or Early Helladic III. It is nevertheless possible that the technique of drilling cylindrical shaftholes in stone and the idea of apsidal houses spread to the Balkans from Anatolia.

The occurrence of both shaft-hole tools of stone and apsidal houses in the Rakhmani horizon in Thessaly might then conceivably reflect the appearance of forerunners of our three main groups of Anatolian immigrants there during the period of Troy I or earlier. Some Rakhmani pottery seems to display features such as internally rolled (anti-splash) rims which can be related to the pre-Trojan Kum Tepe IB horizon of northwestern Anatolia. The apparent predominance of burnishing on pottery of the Rakhmani horizon could similarly be interpreted as looking towards Anatolia. The distinctive Rakhmani Crusted ware with painted designs on surfaces which had often at any rate been previously burnished might then reflect the presence of Anatolian newcomers, who mixed with the existing population and adapted the Dhimini painted ware tradition of Thessaly to their own burnished ware.

In the Early Bronze Age, House Q at Rakhmani was succeeded by another (P) of similar plan but larger (Pl. 7c). The apses of these two houses at Rakhmani are somewhat unusual in being flat and shallow. Houses with flat apses like these are attested in the Early Bronze Age in Bulgaria (e.g. Georgiev 1961: *Beil.* B at end; 1967: 130, fig. 26), although apses of normal semi-circular shape appear to be at home there during the same period (Piggott 1965: 112, pl. 12b). It looks as if shallow apses like those of the Rakhmani houses also existed alongside ones of semi-circular shape in the Early Bronze Age settlement at Karataş in southwestern Anatolia (Mellink 1968: 251, fig. 3: A and C; Warner 1979: 136, ill. 3: top). Flattened and semi-circular apses similarly appear side by side in houses of the Thessalian Middle Bronze Age at Chasampali (Theochares 1962: 36; Sinos 1971: 37 assigns them to the end of Early Helladic, but the clay vases published from them are clearly Middle Bronze Age) (Pl. 7d). The variation in the shape of the apse from semi-circular to flattened may not therefore have any great significance.

Apsidal houses are attested in the local Early Bronze Age at Sitagroi in eastern Macedonia (Renfrew 1972: 119, fig. 7, 8) (Pl. 7f); and it seems very possible in the absence of evidence to the contrary that they were at home in the Early Bronze Age of our group (1) in central Macedonia as well. There does not in fact appear to be any evidence for house plans of the Early Bronze Age in the central and western parts of Macedonia. But an apsidal house was noted in the earliest level of the Late Bronze Age at Vardina (Heurtley 1925: 29-30, fig. 7; 1939: 33), and it is just conceivable that the type had remained traditional in this region from the Early Bronze Age onwards.

There seems to be no evidence to suggest the existence of apsidal houses in the Macedonian Neolithic. In the Neolithic levels at Servia in western Macedonia small rectangular houses were identified, but none of them appear to have had apses (Heurtley 1939: 43-55, fig. 45). The long houses of the Late Neolithic at Dhimini and Sesklo in Thessaly are not strictly apsidal (Robertson 1943: 25-26) (Pls. 6h, 7a). The large house of this period at Sesklo (Pl. 7a) does indeed have a trapeze-shaped unit not unlike an apse in shape at the end opposite the main door; but this does not connect with the rest of the house, and appears to be a later addition (Wace and Thompson 1912: 64-65, fig. 34; Sinos 1971: 19).

In the Peloponnese no example of an apsidal house can be assigned to a period before the beginning of Early Helladic III. The type of house attested at Lerna and elsewhere in the Peloponnese during the previous Early Helladic II period was totally different in plan and conception, with small rectangular rooms. The roofing system was also probably different; that is, the roofs appear to have been flat or low-pitched instead of relatively high-gabled, as they are likely to have been in the case

of the free-standing long houses of Early Helladic III and Middle Helladic times.

At Thebes in central Greece, however, one of two apsidal houses of the standard long type at home in Early Helladic III has been claimed for Early Helladic II (Dimakopoulou 1975) (Pl. 6g). But the pottery from it does not appear to be standard Early Helladic II ware as found at sites like Eutresis in the same region, and it includes a "Trojan cup" with one handle like those of Lefkandi I (Popham and Sackett 1968: 7, fig. 7, 7, 8) (Pl. 7g). This apsidal house at Thebes had been destroyed by fire, and pottery of standard Early Helladic III type was recovered from the rubbish above the ruin debris.

The precise date of an apsidal house in the region of Plato's Academy near Athens also remains ambiguous (Stavropoullos 1956: 53-54; cf. Vanderpool 1957: 282). This was built of mud-brick above low foundations of river pebbles. It was called Early Helladic by the excavator, who reported that pottery from pits near it and from the fill around and over it was Early Helladic II-III. On the evidence at present available it could therefore, like the apsidal house at Thebes, date from a period contemporary with Lefkandi I in Euboea, if not from a time after the beginning of Early Helladic III at Lerna.

The same ambiguities exist in regard to the apsidal hut built without stone foundations which was noted by Theochares (1954 (1): 105) West of the main settlement at Askitario in Attica. It may be significant that the houses in the settlement itself do not appear to have been apsidal; a horse-shoe shaped structure there was evidently an oven or kiln (Theochares 1954 (1): 105; 1955: 113, 110, fig. 1, 50).

Pottery. The pottery of our first (Macedonian Early Bronze Age) and second (Lefkandi I) groups is clearly Anatolian in character. This is less obvious in the case of the pottery of the third or Early Helladic III group. Nevertheless the features that are new in Early Helladic III pottery can be paralleled to some extent in western Anatolia. The use of the fast potter's wheel, and still more significantly the fashion for monochrome dark burnished surfaces which now replace the glazed washes of Early Helladic II, both point to Anatolia as Rutter (1979) has noted. An analysis of the shapes and other details that distinguish Early Helladic III pottery from that of Early Helladic II for Anatolian parallels is beyond the scope of this paper and would require specialist's knowledge of the material. But a few observations may be hazarded.

A two-handled bowl with everted rim was the standard new Early Helladic III type on many sites in the northeastern Peloponnese, and may have served as a drinking cup and/or food vessel (Caskey 1960: 296-97, pl. 70i) (Pl. 7h); It does not seem easy to parallel in Anatolia, but some examples of a class of two-handled bowls of black and red burnished ware from Beycesultan are not altogether dissimilar (Hood

1971) (Pl. 8a). These come from level XVII, the latest level of EB I at Beycesultan, more or less contemporary perhaps with the end of Troy I although this has been disputed. In the Early Helladic III pottery range of central Greece one-handled cups seem to take the place of the two-handled bowls of the Peloponnese (Pl. 8b). It is easier to find Anatolian parallels for these: compare, for instance, Kadish 1969: 55, pl. 23, fig. 3, with Goldman 1931: 118-19, fig. 160 (Pl. 8c, b).

Distinctive types of handles found on jars in Early Helladic III can be paralleled in the Cyclades and in Anatolia. These include horizontal semi-circular (moon-shaped) lugs with a hole through them set on jar bellies (Wünsche 1977: 63) (Pl. 8d), and horizontal trumpet-lugs similarly placed (e.g. Caskey 1960: pl. 70j; 1966: pl. 52, fig. 8; Plassart 1928: 37, fig. 35). The multi-spouted beer drinking vessel to which Mellink (1969) has drawn attention is a rare but unmistakable type found in Early Helladic III and also in the Cyclades and western Anatolia (e.g. Caskey 1956: 162, pl. 43f, from Lerna; Zafeiropoulou 1970: 430, pl. 373, from the island of Kato Koufonisi East of Naxos; Kadish 1971: 135, pl. 29, fig. 31, from Aphrodisias) (Pl. 8e). There are horizontal trumpet lugs on the belly of the one from Lerna, which has three instead of the more usual four spouts. Mellink (1969) has shown that the type is ultimately of Mesopotamian origin.

A number of vase shapes which are common to the horizon of Troy II in Anatolia, the end of the Early Bronze Age on the Greek mainland, and the early part of the Middle Minoan period in Crete, may have been independently inspired by metal types which originated further East in Mesopotamia and Syria. The spouted jar with basket handle is a possible example (e.g. Kadish 1971: 138, pl. 29, fig. 34, from Aphrodisias; Blegen 1928: 104, fig. 89; 123, fig. 115, from Zygouries) (Pl. 8f). Metal jars of this type have been recovered at Troy with the treasures lost at the end of Troy II and elsewhere in Anatolia. A clay example with a strap handle especially suggestive of metal comes from a context of late Early Helladic II at Askitario in Attica (Theochares 1954 (2): 68, fig. 7 right; 71, fig. 18). It will be suggested below that the end of Early Helladic II in Attica may have overlapped with Early Helladic III in the Peloponnese and central Greece.

The "ouzo cup", a variety of handleless goblet at home in the early part of Early Helladic III at Lerna, may be another of these metal-inspired shapes (Caskey 1955: 37, 43, pl. 21e-g; 1960: 296) (Pl. 8g). It is interesting and perhaps significant that the handleless goblets or tumblers made on the fast wheel common at Knossos in Crete throughout the early part of the Middle Minoan period are closely similar in shape and size (Evans 1935: 98-99, figs. 63-65; 130, pl. 30A) (Pl. 8h). In the opinion of Evans (1935: 98) these Knossian goblets were copied from metal ones. Metal goblets from the Royal Cemetery at Ur have the same tall splayed shape as some

of the more elegant of the Knossian goblets and the Lerna ouzo cups (Woolley 1934: 520, pls. 162, 235, types 42, 43) (Pl. 8i).

The distinctive patterned ware of Early Helladic III is not easily paralleled outside mainland Greece. Some of the most characteristic motifs, however, like the groups of parallel lines with fringes, are reminiscent of ones that appear on incised ware in the Cycladic islands (e.g. Atkinson *et al.* 1904: pl. 5, 10, from Phylakopi; Rubensohn 1917: 40, fig. 37, from Paros) (Pl. 9a). This Cycladic incised ware found its way to mainland Greece and the island of Aigina (e.g. Müller 1938: pl. 2, 2; Mylonas 1959: fig. 161; Wünsche 1977: 55-59, fig. 36; 121: nos, 53, 54, pl. 9f, g). The Early Helladic III patterned ware of central Greece is characteristically in white paint on a dark washed ground, while that of the Peloponnese is normally executed in dark paint on a light ground. This suggests the possibility that the fashion for patterned ware developed on the Greek mainland after the beginning of Early Helladic III there. That is to say, Early Helladic III immigrants may have learnt the technique of painting decoration on clay vases from the previous inhabitants, and translated the incised designs with which they were familiar into light paint on a dark ground in central Greece, and dark paint on a light ground further South in the Argolid.

Other aspects of Early Helladic III culture. There are Anatolian parallels for other features of Early Helladic III besides architecture and pottery. Thus seals made of clay were current at Lerna in Early Helladic III, but not it seems in Early Helladic II (Wiencke 1969: 509-21). These clay seals seem to point to Anatolia, where seals made of clay were at home from much earlier times. A clay seal from the earliest level assigned to Middle Helladic at Asine is of cylindrical shape with a string-hole perforation at one end and incised designs round the sides (Frödin and Persson 1938: 239, 235, fig. 172, 9) (Pl. 9b). It closely resembles clay seals found by Schliemann at Troy and assigned by him to his Third or Burnt City which represents the final destruction level of Troy II (Matz 1928: 255-56, fig. 112; Schliemann 1880: 414-15, no. 499) (Pl. 9c).

Changes in types of spindlewhorls, clay reels and loomweights, could be significant in assessing the character and scale of an immigration, since the kind of work (spinning and weaving) for which they were used was usually the sphere of women in early times. A spindlewhorl with sunk top and incised decoration from Lerna IV (Early Helladic III) is of a type at home in western Anatolia in the period of Troy II and later (Caskey 1955: 37, pl. 22, 1). But this may prove to have been an isolated import, as the knobbed bone, some kind of idol perhaps, exactly like one from Troy but with Sicilian relatives, found in a late deposit of Lerna IV, appears to have been (Caskey 1954: 22, pl. 9g; 1960, 297).

The curious double-hook shaped objects known as "anchor ornaments", current

in Macedonia and Thessaly during the Early Bronze Ages there, and in Early Helladic III in southern Greece, may have been articles of weaving equipment, or amulets or idols (J. D. Evans 1956: 99-100; Bernabò-Brea 1964: 588-89; Trump 1960) (Pl. 9d). Anchor ornaments do not seem to occur at Troy, but they have been recovered at Poliochni on Lemnos in levels of the Blue Period, which appears to correspond to the early part of Troy I (Bernabò-Brea 1964: 588-89, pl. 83). Moreover, single clay hooks, which may be related to the double-hook anchors and which are found alongside them in the Macedonian Early Bronze Age and in the Blue Period at Poliochni, are already attested at Poliochni in the Black Period belonging to the horizon of Kum Tepe IB (before Troy I) in the Troad (Bernabò-Brea 1964: 588, pl. 33e, f, h). It would therefore seem that anchor ornaments were earlier in the region of northwestern Anatolia than they were in Macedonia or Thessaly or further South on the Greek mainland.

Some anchor ornaments have indeed been reported from Early Helladic II contexts on the mainland of Greece. But the evidence for their being at home in Early Helladic II is hardly conclusive as yet according to Howell (1973: 83; but contrast Dickinson 1977: 34). At Lerna at any rate they are not attested before Early Helladic III. In Macedonia they do not seem to occur before the beginning of the local Early Bronze Age, and they were especially common in the lowest levels at Kritsana, which is recognised as being the oldest Early Bronze Age settlement so far known in central Macedonia, and therefore the earliest one of possible Anatolian immigrants (Heurtley 1939: 87, 22, fig. 21). From Macedonia anchor ornaments seem to have spread along with pottery of Macedonian Early Bronze Age type, and presumably with the people who made it, into Thessaly, where they are attested from the second part of phase II of the local Early Bronze Age and into the early part of the Middle Bronze Age at Argissa (Hanschmann and Milojčić 1976: 94-95) (Pl. 9d). Hammond (1976: 109) similarly identifies the spread of anchor ornaments with that of an 'anchor people', but regards their presence at Lerna in Early Helladic III as evidence for the derivation of some of the newcomers from the North.

It is usual to emphasise the relatively barbarous character of the Early Helladic III culture of central and southern Greece. This view is based to a large extent on an assessment of the pottery, which is certainly less fine in quality than that of the preceding Early Helladic II period. But a certain dignity and character is reflected in the shapes and the style of decoration of Early Helladic III pottery, and the use of the fast potter's wheel marks a distinct technical advance. Moreover, for what it is worth, signs continue to be incised, or in Aigina painted, on clay vases as they had been in Early Helladic II (Wünsche 1977: 45-46, fig. 31, 118-19, no. 42).

Other arts may have been more flourishing in Early Helladic III than appears

from the hazards of excavation. The jewelry, for instance, alleged to have been found at Thyreatis in Arcadia and now in Berlin, looks too evolved to be a product of Early Helladic II to judge from what little is certainly known of the jewelry of that period (Greifenhagen 1970: 17-18, pls. 1, 2). It shows affinities, however, with the jewelry from treasures lost at the time of the final destruction of Troy II, which may fall within and perhaps towards the end of Early Helladic III. The finger of Authority points in the direction of an Early Helladic III date for it. Thus Zahn (1932: 12, n. 8) placed the Thyreatis jewelry around 2000 B.C. Higgins (1961: 198) thinks that internal evidence suggests a date for it towards the end of Early Helladic, and Bielefeld (1968: 16) firmly assigns it to Early Helladic III but without advancing any specific reasons. It seems at least possible therefore that the Thyreatis jewelry was in fact a product of the Early Helladic III period in Greece.

Admixture of Early Helladic III immigrants with the previous inhabitants. Even if there was an invasion or immigration on a relatively massive scale into the Central and Southern parts of the Greek mainland at the end of the Early Helladic II period, it is unlikely that all the previous inhabitants were slaughtered or expelled. Some of the vase shapes current at Lerna in Early Helladic III are said to represent survivals of Early Helladic II ones (Rutter 1979). The sauceboat, which was probably the standard type of drinking cup in use in Early Helladic II (Weinberg 1969: 5-6, pl. 3, 3; Renfrew 1972: 284; Hood 1978: 33), vanishes without trace at Lerna. But sauceboats were still apparently being made, although they were rare, at Eutresis in central Greece in Early Helladic III (Goldman 1931: 116, 123). This might reflect a greater degree of admixture with elements of the previous population in that area.

As Early Helladic III groups moved further West they may have mixed to an even greater extent with the previous inhabitants. In due course bearers of Early Helladic III culture seem to have penetrated into Elis (Schachermeyr 1971: 391-92, fig. 77, for E. H. III vases associated with anchor ornaments from Olympia; cf. Howell 1975: 111); and they also reached Ithaca, where traces of them have been recovered at Pelikata (Heurtley 1938). In view of the two-handled bowls in use at Pelikata it has been inferred that the Early Helladic III group which settled there derived from the Peloponnese rather than from central Greece (cf. Heurtley 1938: 40). This Early Helladic III material is said to have occurred in the earliest deposits at Pelikata, and it was associated there with some fragments of sauceboats. Such fragments, however, could have come from a previous Early Helladic II settlement, of which no intact deposits were discovered in the excavations.

There are no signs of an Early Helladic III presence in the South or South West of the Peloponnese, in Laconia or Messenia. Here and in other regions where Early Helladic III groups did not immediately penetrate, the existing inhabitants no doubt

continued for a time at any rate to make the kinds of pottery which were traditional with them (cf. Rutter 1979). In areas where the Early Helladic II culture had flourished this would mean that sauceboat drinking cups and other types of Early Helladic II vases remained current during the period defined as Early Helladic III at Lerna (cf. Renfrew 1972: 54). One important area where this may have happened is Attica.

The problem of Attica. The virtual absence of recognisable Early Helladic III pottery in Attica is striking. In her careful publication of the prehistoric material from the Athenian Agora, Immerwahr managed to identify three vases and a few sherds classifiable as Early Helladic III. But she emphasised that 'the small quantity and imported nature of this material should be noted' (Immerwahr 1971: 58). In view of this it is tempting to suggest that the inhabitants of Attica at the end of Early Helladic II (in terms of Lerna) succeeded in retaining their independence and repelled attempts at absorption for a time at least. In that case occupation at Early Helladic II sites like Ayios Kosmas on the South coast of Attica, or Askitario and Rafina on the East coast, might have continued with a repertory of Early Helladic II vase shapes and fashions into the Early Helladic III period as defined at Lerna. Some of the vases from these places look as if they could have been influenced by a sight of Early Helladic III ones (e.g. Mylonas 1959: 46, no. 78, fig. 134, from Ayios Kosmas; Theochares 1953: 113-14, fig. 6, from Rafina) (Pl. 9e). If this was indeed the situation, the final destruction of many of the Attic settlements with Early Helladic II culture might have taken place, not at the end of the Early Helladic II period in terms of Lerna, but towards the end of Early Helladic III, when evidence from Lerna and elsewhere suggests that there were further upheavals in central and southern Greece.

The problem of the R graves on Lefkas. There has been an uneasy feeling that many at least of the burials in the R graves must date from Early Helladic III or even from Middle Helladic times (e.g. Stubbings 1962: 414; Hammond 1976: 125, n. 89). Relatively few clay vases were recovered from the graves, but among them were sauceboats indicative of an Early Helladic II date. At the same time the graves were in tumuli which do not appear to be attested before the Middle Helladic period in other parts of Greece.

The excavator, Dörpfeld, observed that none of the clay vases from the R graves were of the Urfirnis ware coated with a glazed slip characteristic of Early Helladic II (Dörpfeld 1927: 300). He described them as having a thick red-brown slip, and compared the red ware from Thorikos, Aigina, Aphidna, and Tiryns. It may be significant that the red ware from Aphidna at any rate was Middle Helladic, while burnished surfaces in general distinguish the pottery of Early Helladic III from that

of Early Helladic II (Wünsche 1977: 28). The shapes of some of the vases from the R graves, notably pedestal bowls and handled cups, seem reminiscent of Early Helladic III and Middle Helladic (e.g. Dörpfeld 1927: 301, *Beil.* 64: 8, R. 12, 65: 4, R. 16) (Pl. 9f, g).

An astonishing variety of metalwork was found in the R graves (Dörpfeld 1927: 286-95; Branigan 1974: 114; 1975: 37-49). This metalwork is suggestive of an Anatolian, and therefore possibly of an Early Helladic III connection. It included tanged daggers of an Anatolian type of which many examples were recovered at Troy from the horizon of destruction marking the end of Troy II (Åberg 1932: 149-50, figs. 283, 284) (Pl. 9h). A silver bracelet closely resembles a gold one from the great Treasure A lost at Troy then (Dörpfeld 1927: *Beil.* 60: 6; Åberg 1932: 150-51, figs. 287, 288).

It remains to consider the tumuli associated with the R graves. If the practice of tumulus burial was something introduced into Greece from the North, the R graves on Lefkas might reflect the presence of a mixed population, in which Northern elements were combined with ones of ultimately Anatolian and others of native Early Helladic II derivation. In that case it seems just possible that none of the burials in the R graves dates from a time before the beginning of Early Helladic III at Lerna.

Tumuli. The tumuli of the Middle Helladic period in Greece are usually considered to be of northern origin and to reflect the arrival of elements from Europe to the North there. It has been argued, however, that tumulus burial was not in fact a practice introduced from the North, but was independently developed in Greece or brought from the East (e.g. Andronikos 1962: 174). Burial under circular mounds is attested in many parts of the world in early times. The piling of a circular mound over the grave was standard practice in Egypt for instance at the beginning of the Old Kingdom (e.g. Emery 1961: 147, 150, fig. 88).

Some curious raised structures with burials are documented from the Neolithic of Greece. Hammond (1972: 262, n. 14; 1976: 107, 124, n. 32) has suggested that one at Chaeronea described by Sotiriades (1905: 120-29; 1908: 70-94; 1909: 123-5) was in fact a burial mound or kurgan of northern derivation. But Milojčić (1976: 6) has compared this with a Late Neolithic complex which he excavated at Ayia Sofia in Thessaly. The structure at Ayia Sofia was inside the settlement and was not strictly a burial mound. In this case at any rate there does not seem to be any connection with the tumuli of the Bronze Age in Greece, and it seems doubtful if the structure at Chaeronea, about which only the rather confusing accounts left by Sotiriades are available, has any such connection.

There is other evidence, however, apart from tumuli suggesting the possible arrival of elements from the North in Greece towards the end of the Early Bronze Age.

Corded ware. It is widely agreed that this distinctive type of pottery, which makes a sporadic appearance in some parts of the Greek mainland during the Early Bronze Age, is of northern origin. The rare occurrences of it in Greece have been listed by Hanschmann and Milojčić (1976: 231-35) and by Hammond (1976: 111). But the examples which Hammond notes from Aphiona in Corfu and from Lefkas do not appear to be strictly corded ware, and the fragments with cord impressions from Epirus might date from a very much later period, since cord-ornamented pottery was again current in some parts of the Aegean area at the end of the Bronze Age; it is found, for example, at Troy alongside the intrusive Knobbed Ware of Troy VIIB: 2.

A small amount of typical corded ware has been recovered from contexts of the local Early Bronze Age in Thessaly. At Pefkakia five fragments of it are reported from deposits assignable to the end of the Early Bronze Age (Early Thessalian III) which succeeded the stage (Early Thessalian II) marked by the appearance of our second (Lefkandi I) group of possible Anatolian immigrants with their wheelmade pottery of Trojan type and long houses. But two other fragments were probably from an earlier (Early Thessalian I) context at Pefkakia. Three fragments of vases and a spindlewhorl with cord impressed decoration were similarly recovered from deposits of Early Thessalian I at Argissa further North. This corded ware might have been imported into Thessaly: but its occurrence at Argissa in the horizon marking the beginning of the Early Bronze Age, coinciding as it does with a sharp break in the pottery tradition there, could be significant.

It was suggested above that the Early Bronze Age in Thessaly was inaugurated by groups of ultimately Anatolian origin spreading from western Macedonia and introducing a fashion for pottery of Macedonian Early Bronze Age type. The presence of some corded ware alongside this Early Bronze Age pottery of Macedonian type at Argissa, and also probably at Pefkakia, might reflect a northern element among the newcomers, imposed perhaps as a ruling class upon them. At the time of their arrival in Thessaly some of the Northerners may have still clung to their traditional cord-ornamented pottery, which they had brought with them, or which they continued for a time to make.

In central Macedonia, however, only two fragments of corded ware have been reported from contexts of the Early Bronze Age, one from Kritsana, the other from Ayios Mamas (Hanschmann and Milojčić 1976: 231-35). But a good deal appears to have been recovered from levels of the local Early Bronze Age at sites in eastern Macedonia, at Dikilitash where it seems to occur at the beginning of the Early

Bronze Age but is well represented in Phase IIb, and at neighbouring Sitagroi where it is first attested in Phase Va of the settlement there (Renfrew 1972: 120). Corded ware appears to be still more at home in the related Early Bronze Age of Bulgaria. Thus at Ezero it occurred at the beginning of the Early Bronze Age, and it was very widespread throughout the middle Early Bronze levels (Georgiev 1967: 124; Georgiev and Merpert 1966: 34).

Very little corded ware has so far come to light in central or southern Greece. Scraps of it were found at Ayia Marina and Eutresis, and Milojčić (1956: 159) has drawn attention to part of a jar or bottle from Corinth (Pl. 9i) assigned by Walker Kosmopoulos (1948: 17, fig. 2) to the Neolithic. The only corded ware from a stratified deposit in this area is a scrap allegedly from a context of the end of Early Helladic III at Eutresis (Goldman 1931: 123). The amount of this corded ware found in central and southern Greece is so minute that it could all have been imported. But it is difficult to see why the people of this region in the Early Helladic or any other period should have wanted to import crude cord-ornamented wares from the North. It seems easier therefore to suppose that it may reflect the presence of immigrants or invaders.

Hollow-based stone arrow-heads. The scrap of corded ware is not the only object of probable Northern derivation alleged to come from a context of Early Helladic III at Eutresis. There was also a hollow-based obsidian arrow-head, a type which was to remain standard on the Greek mainland throughout the Middle Helladic period and into Mycenaean times (Goldman 1931: 206, 205, fig. 276, 4; Dickinson 1977: 35) (Pl. 10a). Hollow-based stone arrow-heads were very much at home in Europe to the North of Greece; but they do not appear to have been current in Anatolia. Only one seems to be attested at Troy, and this comes from a relatively late horizon (Troy Va) and might well have been an import (Blegen *et al.* 1951: 223, 256, fig. 234, 36.116). Nor do arrow-heads of this type appear to be found in the Greek Neolithic, when a tanged arrow-head was characteristic (Belmont and Renfrew 1964: 399-400). In Macedonia and Thessaly the hollow-based arrow-head does not seem to occur before the local Early Bronze Age (Heurtley 1939: 86, 200, fig. 65x, z; Tsountas 1908: 325, pl. 42, 11-13: note that two of these which Tsountas says were from definite Early Bronze Age contexts are wrongly assigned by Buchholz 1962: 31, his Type III, to the Neolithic).

There is a possibility, however, that arrow-heads of the hollow-based type were already in use in southern Greece in Early Helladic II. A neat hollow-based arrow-head from Malthi (Valmin 1938: 357-58; 29, fig. 8, 8) is illustrated among finds from one of the early apsidal houses there; but, as noted above, occupation at Malthi does not appear to have begun before Middle Helladic times. An obsidian arrow-

head of this type from the fire-destroyed Early Helladic II 'House of the Pithoi' at Zygouries might just possibly have been a later (Mycenaean) stray (Blegen 1928: 199, pl. 20, 23). But two fine obsidian arrow-heads with hollow bases were recovered from Early Helladic II contexts at Ayios Kosmas in Attica (Mylonas 1959: 17, 28, 30, 144, fig. 166, 17, 18). It has been suggested above, however, that the Early Helladic settlement at Ayios Kosmas might have continued in existence into a period contemporary with Early Helladic III at Lerna.

Another item of archery equipment which appears to have been introduced into Greece from Europe to the North is the grooved stone arrow-shaft straightener or smoother (Buchholz 1962: 5-20). Stone arrow-shaft smoothers are not certainly attested in Greece before Early Helladic III or Middle Helladic times, and Buchholz (1962: 20) has looked for their origin in South Russia, home of the Kurgan (Yamnaya or Pit-grave) culture.

It is tempting to believe that the introduction of the hollow-based arrow-head to Greece coincided with that of the arrow-shaft smoother. If they in fact go together in this way they might reflect the appearance of a new type of arrow-shaft—one made from a stick, which would need smoothing or shaping, instead of from a reed. A reed would not require such treatment, and being hollow it would be naturally adapted to fit the tang of the type of arrow-head current in the Aegean area during the Neolithic there.

Tumuli and corded ware. The tradition of ornamenting pottery with cord impressions and the practice of burial under tumuli appear to have originated in different regions of Northern and Eastern Europe, although the custom of tumulus burial, deriving it seems from the steppes of South Russia, was at an early stage adopted by people making cord-ornamented ware (Sulimirski 1970: 150). It is therefore possible that the practice of burial under tumuli was introduced into Greece from the North by users of corded ware. But there is not as yet any clear evidence from Greece to suggest this, and the evidence, such as it is, might seem to indicate that the presence of corded ware there had no connection with the introduction of tumulus burial. No tumuli of Early or Middle Bronze Age date have yet been recorded from Thessaly or Macedonia, although that does not mean that tumuli of those periods may not one day be discovered there. Hammond (1967: 94-96; 1972: 243-44) has indeed suggested that a burial found by Heurtley in a level of the Early Bronze Age at Servia in western Macedonia was in a tumulus; but this burial appears to have been within the area of the settlement, and no certain traces of a mound over it had survived.

The earliest phase of the local Early Bronze Age (Early Thessalian I) during which corded ware is first attested in Thessaly almost certainly began before, and perhaps

well before, the end of the Early Helladic II period as defined at Lerna. The one stratified scrap of corded ware from Eutresis further South is alleged to have come from a context of the end of Early Helladic III. The R graves on Lefkas with their tumuli may in fact date from Early Helladic II, as the presence of sauceboats in them suggests; but a case has already been made above for assigning them to a later period, Early Helladic III or even Middle Helladic.

Hammond (1972: 248, 263) has reasonably argued that the people who introduced the custom of tumulus burial into the Peloponnese entered it from the North West through what is now Albania. That would be in harmony with an early appearance of tumulus burial on Lefkas, and with its very marked predominance in the western part of the Peloponnese, in Messenia, where by contrast signs of an Early Helladic III presence are lacking. It has indeed been suggested that some of the numerous burial mounds in Messenia (listed by Pelon 1976: 74-78, 462-64) may date from Early Helladic III rather than from Middle Helladic times: notably in the case of the tumulus with pithos burials excavated by Marinatos near Papoulia in the region of Pylos (Marinatos 1954: 311-16; Pelon 1976: 76-77). But this tumulus seems to be Middle Helladic, although perhaps assignable to early in the period (Dickinson 1977: 34; cf. Andronikos 1962: 156).

None of the tumuli that have so far been excavated in the eastern part of the Peloponnese or in central Greece appears to be Early Helladic. It has indeed been claimed that the mysterious brick-built tumulus of Zethos and Amphion at Thebes was constructed in Early Helladic II (Spyropoulos 1972 (1); 1972 (2): 307-308; 1973: 248-52). But the evidence of the sherds incorporated in the mound does not seem conclusive for the date of its construction, and the neat cist-grave below it, as well as the character of the jewelry which may have belonged with the original burials, appear consistent with a later date.

Tumuli of the Middle Helladic period have now been identified at Argos and Asine in the Argolid (Protonotariou-Deilaki 1971: 80; 1973: 95-98; Dietz 1975), and at Vrana near Marathon in Attica (Pelon 1976: 82-85). But tumuli of Bronze Age date do not seem to be very numerous on the Greek mainland outside Messenia, and some of those which have been excavated were in use at the end of the Middle Helladic period and into Late Helladic (Mycenaean) times. A tumulus at Drakhmani in Phocis was exceptional in that it contained a single burial instead of several; this was assignable to the very beginning of the Middle Helladic period (Dickinson 1977: 97-98).

The tumulus at Drakhmani appears to be the only one of the Bronze Age that has been recognised in central Greece, apart from the exceptional brick-built tomb of Zethos and Amphion at Thebes, and a mound at Kamarion in Euboea covering

three plundered cist graves (Papavasileiou 1900: 65; 1910: 45. This may have been Middle Helladic (Dickinson 1977: 98) rather than Late Helladic (Sackett *et al.* 1966: 62, n. 54) or Submycenaean/Early Iron Age in date (Philippson 1951: 606, 722)).

The end of Early Helladic III. The Early Helladic III settlement at Eutresis in central Greece was destroyed by fire. Destructions assignable to the end of Early Helladic III have also been claimed for sites in the Peloponnese, and while some of these claims may be doubtful, this does not seem to be so in every case. At Lerna in the Argolid there were marked signs of dislocation at the end of Early Helladic III, although no evidence for a major destruction. At Berbati, however, in the same region, the settlement does not appear to have been destroyed at the end of Early Helladic II as that at Lerna was, but there is evidence for a large-scale destruction there by fire at the end of Early Helladic III (Immerwahr 1966). Other sites which may have suffered destruction by fire about the end of Early Helladic III are Korakou near Corinth (Blegen 1921: 2; Caskey 1960: 302), Tsoungiza close to Nemea (Harland 1928), and Asea in Arcadia (Holmberg 1944: 11). It has already been suggested that some of the settlements in Attica like Ayios Kosmas with ostensibly Early Helladic II culture might have been destroyed at this time rather than earlier.

There is nothing to indicate that this horizon of destruction and dislocation at the end of Early Helladic III in central and southern Greece was associated with the appearance of corded ware in that area. In Thessaly the local Middle Bronze Age was apparently inaugurated by a movement of people from Macedonia about the same time as, or even, if Hanschmann and Milojčić (1976: 226, fig. 11) are right, slightly before, the beginning of Middle Helladic further South. This later movement from Macedonia into Thessaly does not appear to have been associated with corded ware as was the earlier movement which inaugurated the Thessalian Early Bronze Age.

A scatter of exotic pottery, however, has been reported from Early Helladic III destruction levels and from contexts of the very beginning of the Middle Helladic period in central and southern Greece. It is tempting to connect the appearance of this pottery with the disturbances taking place in those regions at the time, and it would be interesting to know whether some of it at any rate might not reflect the presence of a northern element among the groups who seem to have been moving around in the central and southern parts of Greece then.

Among the vases which have been claimed as exotic on the sites where they have been found are round-bottomed flasks with vertically perforated lugs on the neck. One recovered from the destruction level assigned to Early Helladic III at Asea in Arcadia has a band of incised decoration on the neck and continuing on the lugs (Holmberg 1944: 88, fig. 89a) (Pl. 10b). A plain handmade flask of the same shape was found at Tiryns by Schliemann (1886: 59, pl. 23d) (Pl. 10c).

The most striking examples of this exotic pottery are the little handmade flasks of rather similar shape but with flattened bases and with pairs of double string-hole perforations through the rim in place of lugs. Many of these were apparently found in the earliest levels of the Middle Helladic period at Lerna, and notably in the area of the apsidal house 98A which replaced the latest of the series of possible chieftain's houses of the Early Helladic III period on the east side of the mound over the House of the Tiles (Caskey 1956: 160, pl. 43b; 1957: 150, pl. 40d, f) (Pl. 4e, f). Flasks of this shape, with similar pairs of string-holes through the rim, have been recovered from other early Middle Helladic deposits, notably at Kirrha South of Delphi (Dor 1960: 87-88, pl. 44, 34; Caskey 1962 for the date). At Argos flasks of this type accompanied Middle Helladic tumulus burials (Catling 1974: 10-11, fig. 13; Protonotariou-Deilaki 1971: 80, pl. 67) (Pl. 10d). One is reported from a Middle Helladic apsidal house at Ayios Stephanos in southern Laconia (Catling 1978: 32).

There are also plain flasks assignable to this type, and some of these at any rate look as if they were local products (e.g. Verdelis 1952: 173-74, fig. 10, from Pteleon; Goldman 1931: 141, fig. 193, from Eutresis). One of these plain flasks was found in an early Middle Helladic grave at Asine (Frödin and Persson 1938: 264-66, fig. 184, 8) (Pl. 10e). A comparable string-hole flask is illustrated from an Early Bronze Age lake settlement near Varna in Bulgaria (Margos and Tontchéva 1962: 7, fig. 6, 6); but this appears to have had a rounded instead of a flat base, like a Late Chalcolithic bottle from Beycesultan (Lloyd and Mellaart 1962: 85, 90, 84, fig. P. 6, 14, type 14). The vase from Beycesultan might pass for an ancestor of the type and suggest that the original inspiration for it came from Anatolia. There are also plain flasks of similar shape but with pairs of single instead of double string-holes (e.g. Wace and Thompson 1912: 140, fig. 86e, from Tsani). Flasks of this variety have been found outside Greece in Albania (Hammond 1967: 100 pl. 22, 8, from Libonik, assigned to the local Neolithic) and at Troy (Schmidt 1902: 64, no. 1489).

M. V. Garašanin, who saw much of the relevant flask material from Lerna, is reported to have said that it resembled—or indeed duplicated—certain pots found in deposits of period IA at Bubanj near Niš in Yugoslavia (Caskey 1957: 150). Bubanj IA appears to have been contemporary with the end of the Vinča culture, Vinča D, further north. None of the relevant pots from Bubanj seems to have been published as yet, but channelled decoration on vases from the later horizon at Vinča is reminiscent of that on some of the Lerna flasks (Hood 1970: 84, 89, fig. 4). A fragment of a vase from Hungary assigned to the Balaton group, which appears to be contemporary with the final phase (D) of Vinča, has a design of channelled concentric circles against a background of parallel channels closely resembling the decoration on one of the flasks from Lerna (Kalicz 1973: 135, fig. 2, 4; 139, fig. 5, 2; cf.

Hood 1970: 88, figs. 1, 2; 1973: 125, fig. 7, 4, 5 (Caskey 1957: pl. 40d, f)) (Pls. 10f; 4e, f). But it must be admitted that on a calibrated C-14 chronology for the Balkans the horizon of Bubanj IA and Vinča D would have to be set well before 3000 BC, that is, a thousand years or more before any reasonable date for the beginning of the Middle Helladic period in southern Greece. Moreover, parallels for the Lerna flasks are also said to exist in the Early Bronze Age Monteoru culture of Romania (Makkay 1976: 269).

The Middle Helladic pottery of central and southern Greece is essentially a development of that which flourished there in Early Helladic III as Wünsche (1977) and others have stressed. The long house with an apse at one end characteristic of Early Helladic III similarly remained standard in that area in Middle Helladic times, and is also found in Thessaly in the Middle Bronze Age there (e.g. Theochares 1962: 35) (Pl. 7d). A feature which occurs at the centre of some Middle Helladic tumuli, and which may have been intended for a common house of the dead, is upon occasion at any rate apsidal (Pelon 1976: 111-12; Calligas 1977).

The evidence therefore suggests an essential continuity from Early Helladic III to Middle Helladic in both pottery and housing. When it comes to funerary customs, however, there seems to have been an important innovation with the appearance of tumulus burial. But burial in single-graves, often inside the settlement area as at the key sites of Lerna and Eutresis, but also in cemeteries beyond the settlement limits, is attested alongside burial in tumuli. It is possible that single-grave burial, whether inside the settlement area or outside it, was a tradition inherited from Early Helladic III. Burial in single-graves was standard practice in the Early Bronze Age in Anatolia, in cemeteries outside the settlement in the West, but within the settlement in parts of the hinterland (Wheeler 1974).

Early Helladic III burial customs. There seems to be very little information of any kind available about Early Helladic III burials. What little is known, however, suggests that burial was in single-graves, and sometimes apparently in pithoi as was customary in western Anatolia (Wheeler 1974). This represents a radical change from Early Helladic II funerary practice with collective burial in rock-cut or stone-built chambers.

A rock-cut cistgrave covered with slabs inside the area of settlement at Tsoungiza near Nemea assigned by Harland (1928) to the Early Helladic period might have been Early Helladic III. Two graves of adults inside the settlement at Ayia Marina in central Greece might similarly have been of Early Helladic III rather than Middle Helladic date (Syriopoulos 1968: 231). A pithos burial within the settlement area at Pelikata on Ithaca was probably Early Helladic III (Heurtley 1938: 14, 18, 19).

Two single-graves of adults which certainly date from Early Helladic III have

been identified in flat ground by the Agricultural Prison about 150 metres South of the Acropolis at Tiryns (Verdelis 1956: 4, fig. 6; French 1971: 24). French (1971: 28) has emphasised that there does not appear to be any settlement stratification in the area of the graves, and the pottery from it assignable to Early Helladic III (apart from the vases from the graves (Pl. 10g)) is just a few sherds (French 1971: 23-24).

The evidence, scanty as it is, therefore suggests that Early Helladic III burials were normally in single-graves, and at times in pithoi as in western Anatolia; it opens the door to the possibility that in some instances at any rate burials were made within the settlement area. The circumstances at Lerna and Eutresis at the beginning of the Middle Helladic period might conceivably be interpreted as reflecting a partial replacement of original Early Helladic III populations which did not practice burial inside the settlement by related groups from other regions which did, with the possibility that these intrusive groups were under the leadership or control of Northern elements.

Summary. The aim of this paper has been to suggest that there were two separate movements of people into the Greek mainland during the Early Bronze Age: one on a relatively massive scale, and spread over a considerable period of time, from Anatolia, the other from Europe to the North of Greece. Each of these movements was complex, and in many areas they became intertwined.

The three main groups into which the Anatolian movement is divisible may have shared some features in common, notably the building of apsidal houses, which appear to have been at home in some parts of Anatolia before they were in South-East Europe. The use of the fast potter's wheel and of distinctive clay anchor ornaments were also shared to some extent by groups of the Anatolian movement.

The Northern movement seems to be reflected in the spread of the practice of tumulus burial, and less ambiguously in the appearance of hollow-based stone arrow-heads and arrow-shaft smoothers, as well as of certain types of exotic pottery including corded ware.

There is no evidence as yet in Greece for connecting the appearance of corded ware with that of the practice of tumulus burial, and they may reflect different groups which successively entered Greece from the North.

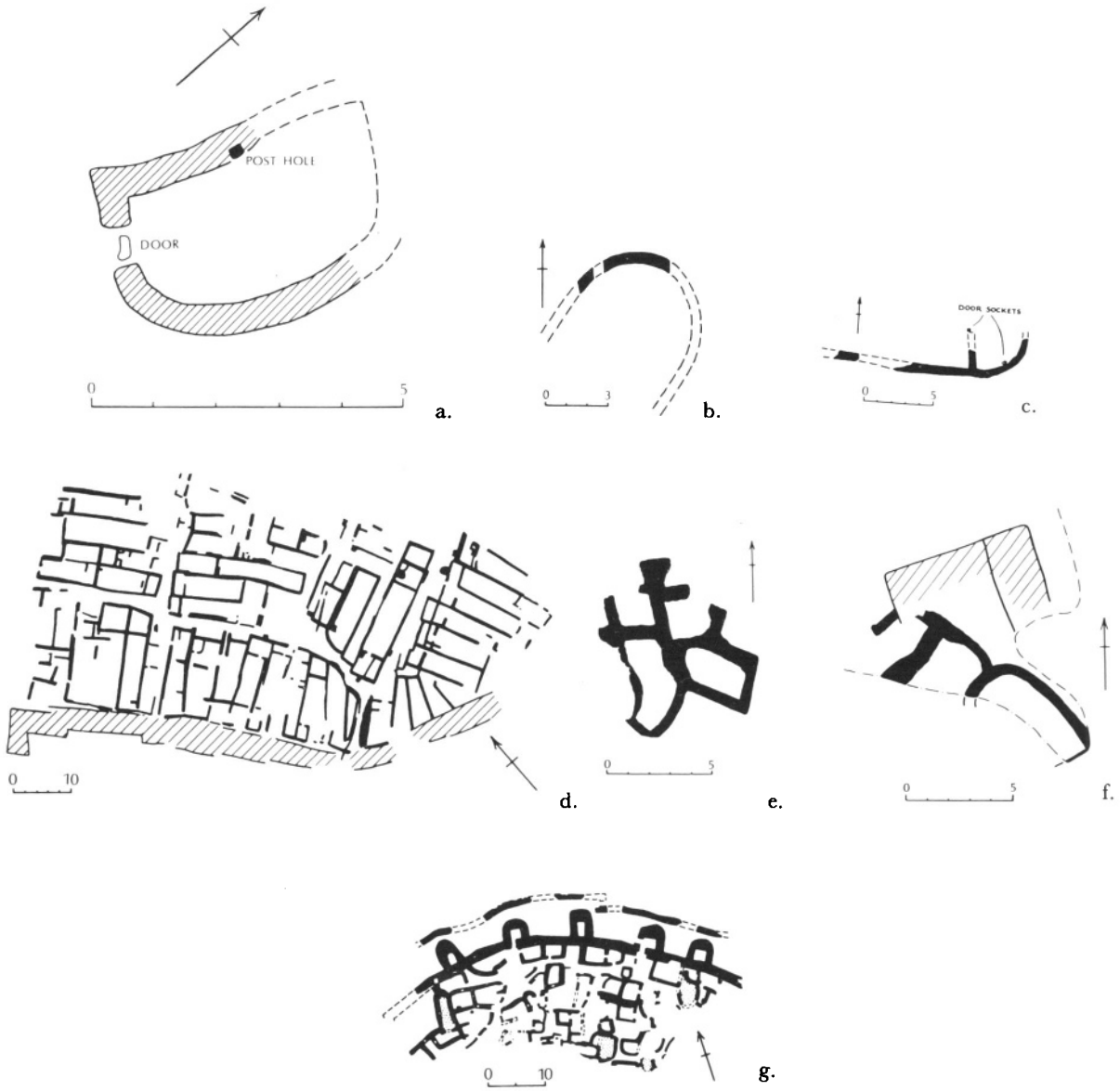

Plate 5

a) Chios, Emporio VIII: Early D-shaped house; b) Poliochni, Black Period: Curving wall restored as an apsidal house; c) Troy Ia; d) Thermi V; e) Poliochni, Blue Period: Apsidal vestiges; f) Poliochni, Blue Period: Apsidal vestiges; g) Chalandriani, Kastri: Apsidal vestiges.

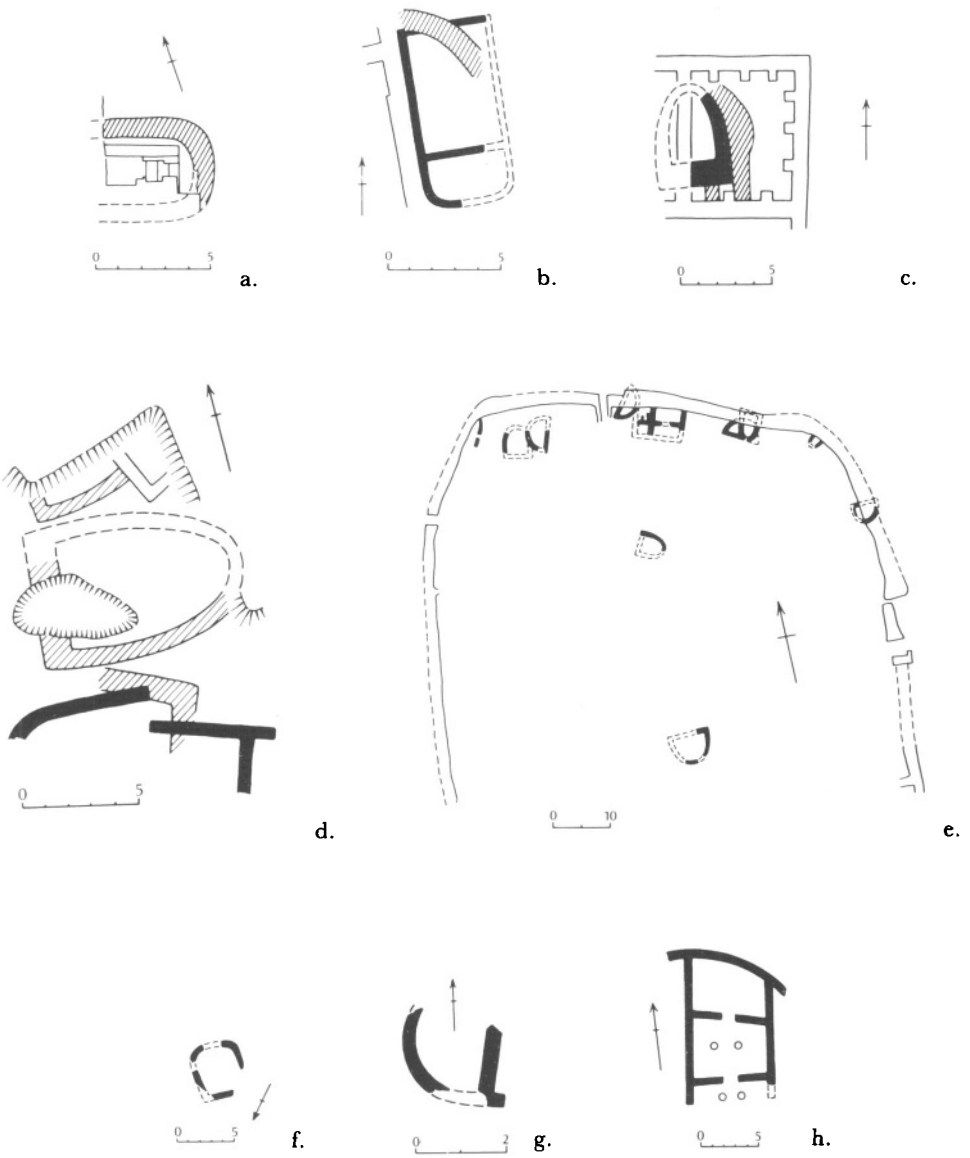

Plate 6

a) Paros, Paroikia; b) Paros, Pyrgos; c) Delos, Mt. Kynthos: House Psi; d) Orchomenos, bothros horizon; e) Malthi, early levels; f) Krisa, Middle Helladic; g) Thebes, EH II; h) Dhimini: Late Neolithic megaron.

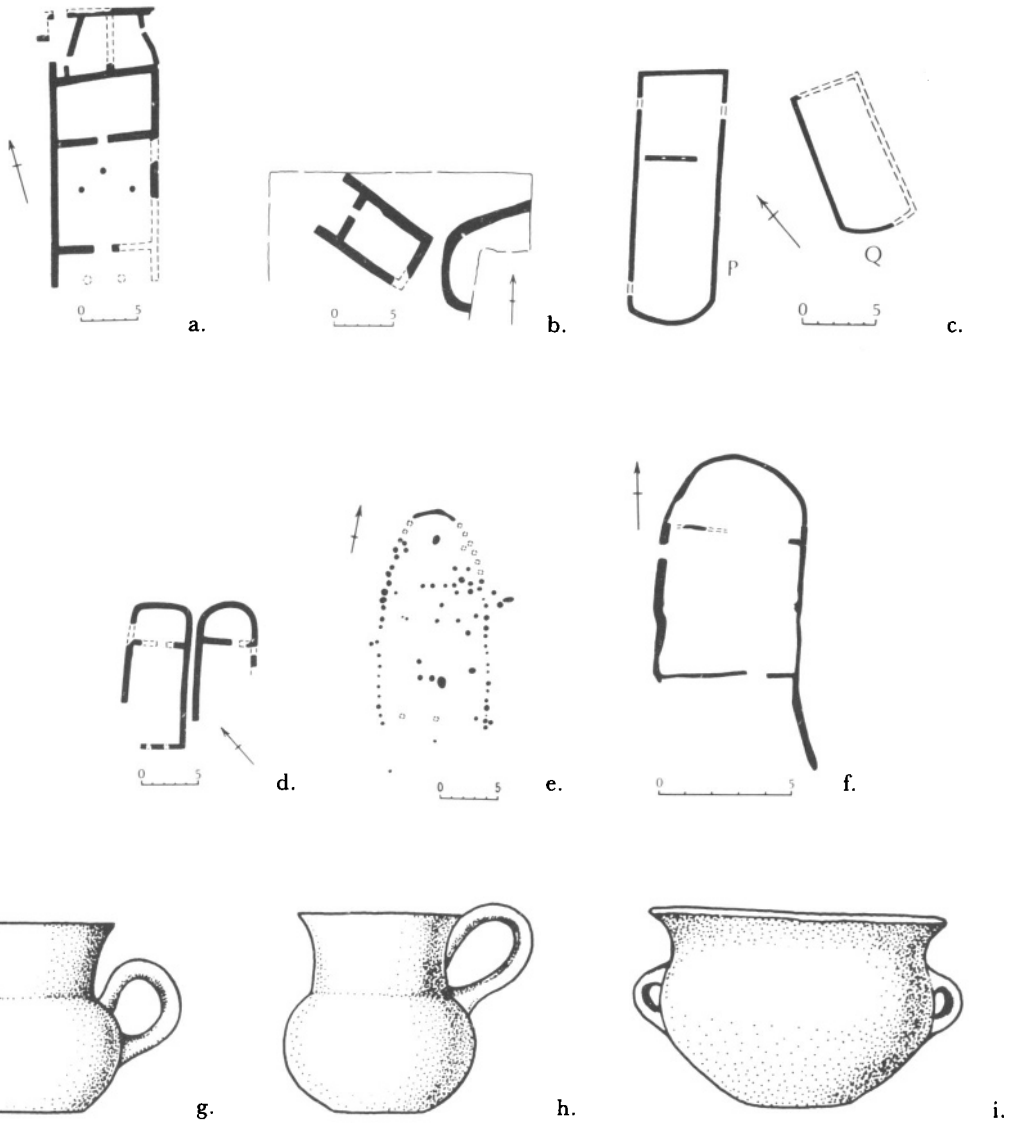

Plate 7

a) Sesklo: Late Neolithic megaron; b) Pefkakia, Early Thessalian II; c) Rakhmani: Houses Q and P; d) Chasampali; e) Bylany, assigned to Lengyel; f) Sitagroi, EBA; g) Lefkandi I: "Trojan cups"; h) Lerna: Two-handled bowl, EH III (h. 0.16).

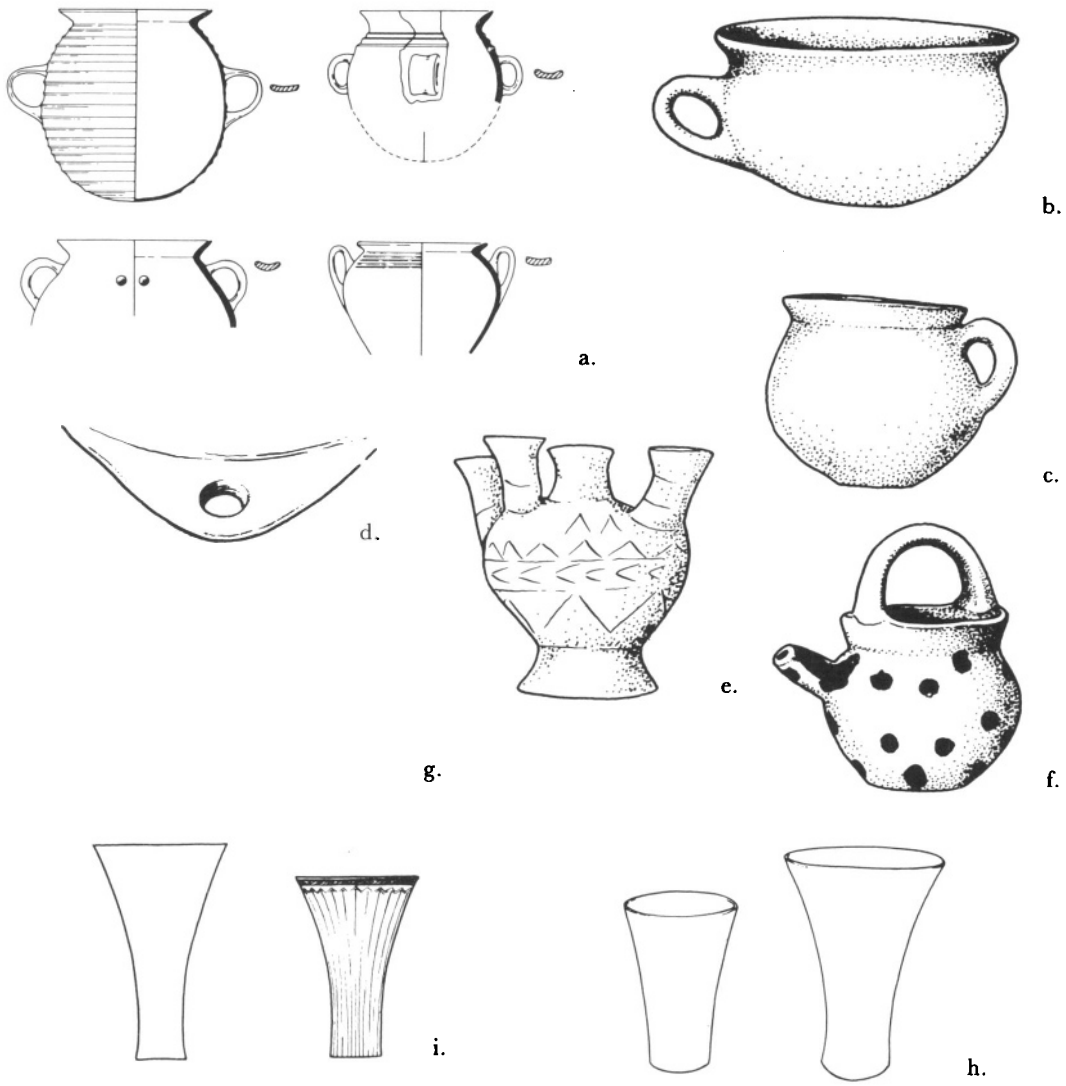

Plate 8

a) (upper row and bottom left) Beycesultan XVII; (bottom right) Knossos, dated to EH III/MH I: Two-handled bowls; b) Eutresis: One-handled cup, EH III (n. 0.19); c) Aphrodisias: One-handled bowl (n. 0.13); d) Aigina: Lug from the belly of a jar, EH III; e) Kato Koufonisi: Multi-spouted vessel (n. 0.125-0.13); f) Zygouries: Spouted jar with basket handle, EH III (h. 0.074); g) Lerna: "Ouzo cups", EH III (h. 0.083 (right)); h) Knossos: Goblets, MM I-II; i) Ur, Royal Cemetery: Metal goblets (h. 0.18 (left); 0.15 (right)).

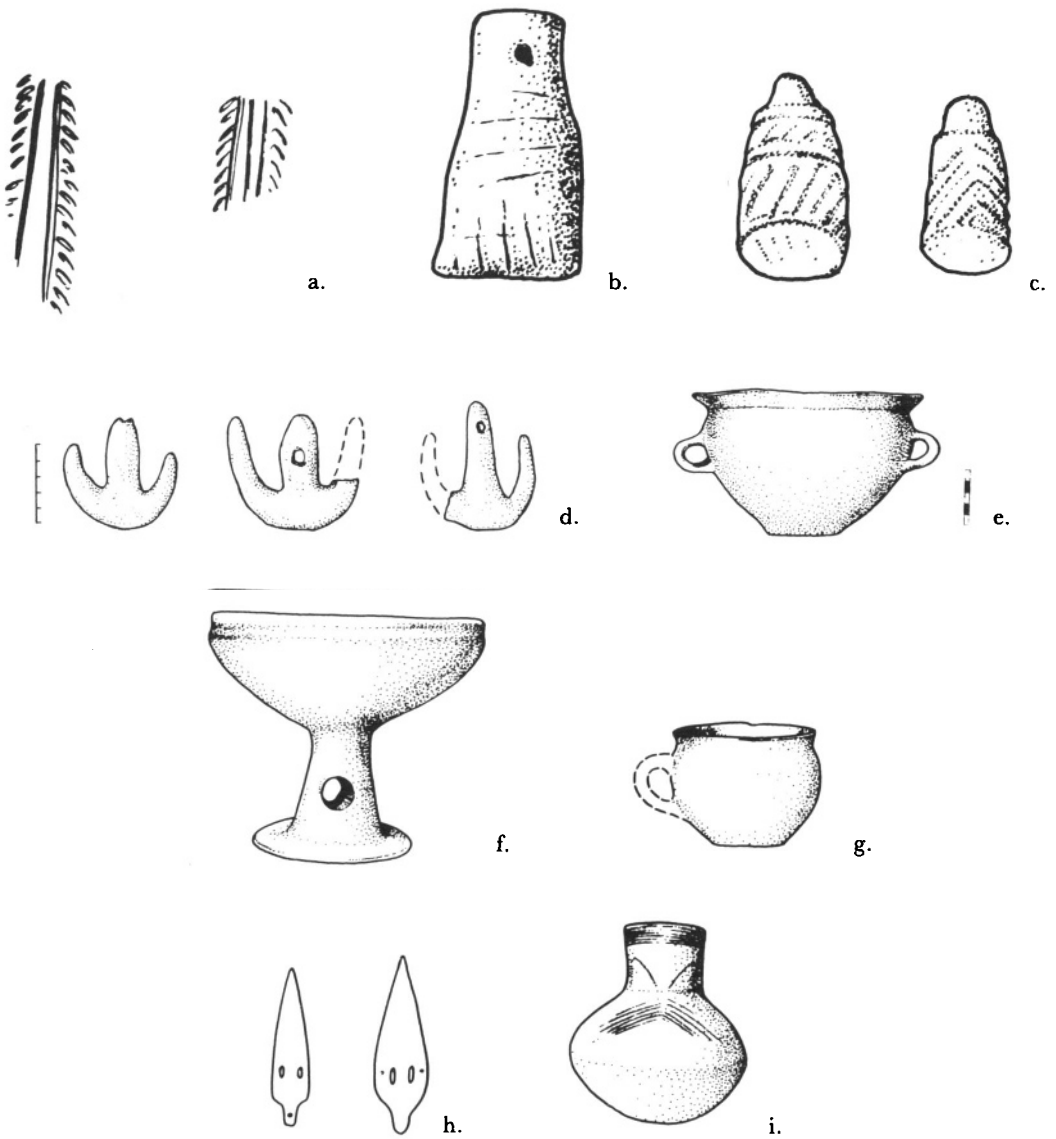

Plate 9

a) Aigina: Incised decoration on pyxis; b) Asine: Clay stamp seal (h. 0.025); c) Troy: Clay stamp seals; d) Thessaly: "Anchor ornaments"; e) Attica, Rafina: Possible imitation of an EH III two-handled bowl; f) Lefkas, R graves: Pedestal bowl (h. 0.205); g) Lefkas, R graves: Cup; h) (left) Lefkas, R grave 24; (right) Troy, Treasure A: Daggers (h. 0.18 (left)); i) Corinth: Bottle with cord ornament.

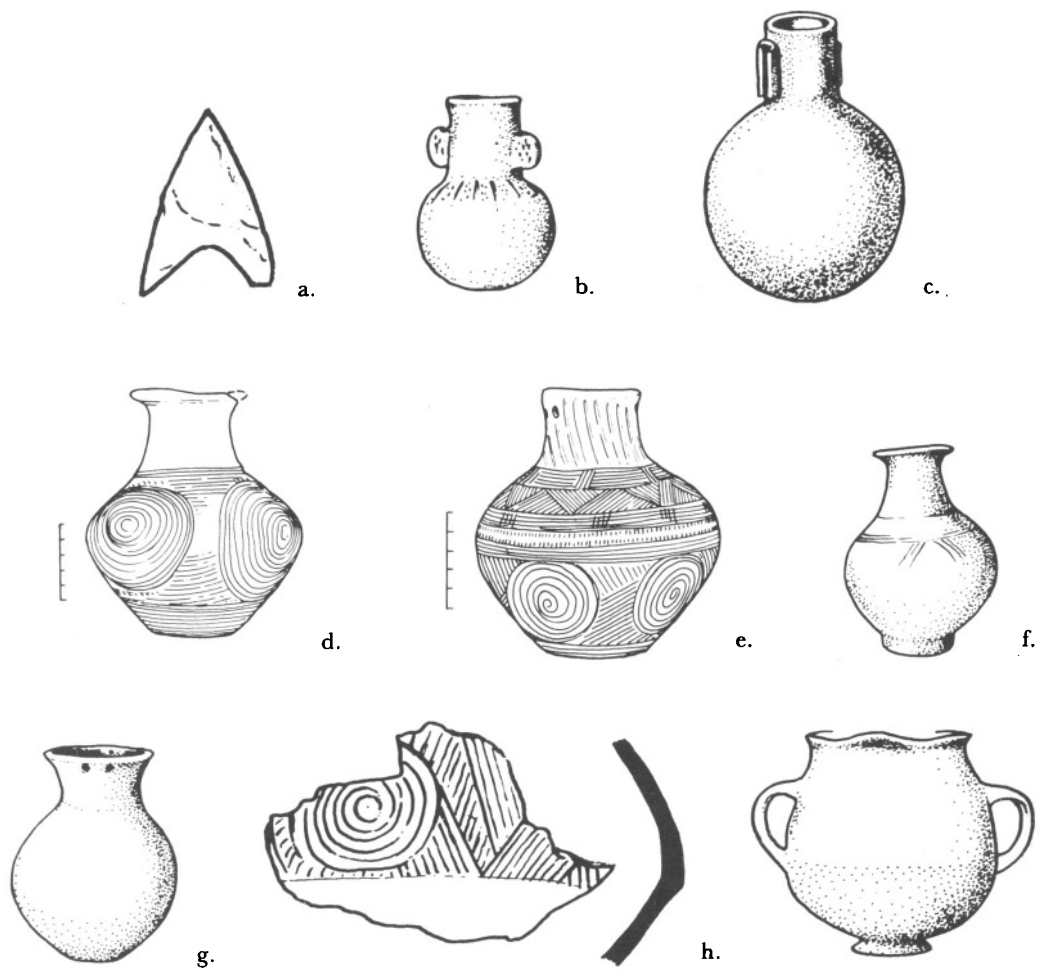

Plate 10

a) Eutresis: Hollow-based stone arrow-head (l. 0.027); b) Asea: Flask, EH III destruction level (h. 0.10); c) Tiryns: Flask (h. 0.11); d) Lerna: Flask; e) Lerna: Flask; f) Argos: Flask; g) Asine: Flask (h. 0.14); h) Hungary: Fragment assigned to the Balaton group; i) Tiryns: Vase, EH III grave.

ART AND THE WORLD OF THE EARLY BRONZE AGE

MARTHA HEATH WIENCKE

(Plates 11-15)

In this paper, I have tried to outline a method for the study of prehistoric art, which I hope may be used to illuminate a little further that period of our past on which the work of John Caskey has already shed so much light. We who have been his colleagues and his students know that we owe him very much indeed, and not least the opportunity to reflect again on the problems of the Early Bronze Age together.

The most compelling reason for considering the visual art of the Early Bronze Age is that we need to know more about it. Perhaps a better reason is that we have a great many objects to consider, whereas we have nothing of the other arts, no oral literature, no religious rites, no recorded history, no written records of any kind. In such a situation, the surviving objects take on a disproportionate importance; we cannot afford to neglect anything, and at the same time we are deprived of much evidence that historians of art in other times and cultures can take for granted. We must rely on purely archaeological evidence which presents us with its own considerable difficulties. Where are we to start?

First, we can deal as scrupulously as possible with the evidence we do have. There is a great deal yet to be done, particularly in excavation, and in the classification and publication of objects.

Secondly, we can draw on the experience of anthropologists and art historians who deal with problems of interpretation elsewhere. This suggestion is not meant to justify unbridled diffusionism or Freudian interpretations. But there is more to the study of any art than the classification of details, and for many periods the relationships between the arts, and between art and society, or religion, or psychology, and the like, have been explored with methods and results which we ought not to ignore.

Next, as we begin to look at some of our material, I should like to propose some axioms for our guidance. First, what are we to consider as art? How do we define it? Are we to look for high art, decorative arts, pure or functional arts? It seems to me that, for our period at least, we do well to consider as art *anything made by human hands which embodies any element of choice in its manufacture.* A piece of bone, for example, need for punching a hole in leather, may be sharpened for the purpose, or a lump

of clay stuck as a weight on the end of a spindle, and that will be the end of it. But if only bones of a certain shape are selected, or if whorls are made conical in one village and round in another, then choice has entered, and art as well.

As a second axiom, let us take it that *all choices are involved with function* (although not with function exclusively), and that *all arts are functional*, that is, the maker has to have a reason for making the object, whether that reason is to propitiate the gods, or to stir the soup. An idol is as functional an object as a spoon.[1]

The third axiom is that *arts cannot be fully understood*, though they may well be studied, *in a vacuum*. That is not to say that any art *is* fully understood, even the best documented; if so profound a student of antiquity as Jacobsen says that he doubts whether we ever really understand ancient literature and art (Jacobsen 1960), we may well hesitate. But whatever small understanding of EBA art we may hope for will not be reached without a consideration of its setting in time, place, and society: setting, that is, in both a visual and a social sense.

If we accept these three axioms (choice, function, setting), at least tentatively, then certain approaches to the study of visual art will follow. We will not at first investigate a single object or class of objects alone (although such studies are of course essential), but in contemporary context; not a single sauceboat or figurine, not all sauceboats without bowls, nor pottery without weapons, but groups of objects as they are found together on a house floor, or in a grave, or in a whole settlement destroyed at one time. Such groups are not so common nor so securely dated as we should like, but they are enough to form a nucleus for the study of art. They need not be large: a pair of schematic figurines and two marble pyxides in one Cycladic grave will tell us more of choice and function than any number of isolated pieces. Any group of this sort—pots of several shapes, a few tools, perhaps some seals or jewelry, or a stone vessel, or a figurine—will form, together with a natural or architectural setting, a kind of visual environment for the makers and users of each single object. And to this visual environment we will add whatever we can reasonably deduce of the invisible environment: the perished arts, the non-visual arts, and the social framework in which the artists worked.

We set ourselves, then, a series of tasks. One is to look closely at all the material found in a group, and at both the architectural setting, if we have one, and the natural setting in order to establish the formal principles governing the choices

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[1] I owe much to the analysis by Renfrew (1972: see especially 406). Compare Kubler 1978: 8: "every man-made thing arises from a problem as a purposeful solution." Kubler, in dealing with the whole span of art history, then distinguishes sharply between tools and art, between crafts and fine arts (14). I think that we cannot usefully maintain such a distinction in the third millennium, however useful it may perhaps appear to be in later times.

which have produced these particular objects rather than others.[2] These formal analyses should furnish us with at least an outline of the style which the group demonstrates, a syntax of its visual language, as students of primitive art have defined style (Fraser 1966: 2; Forge 1973: 189). At the same time, we can with caution assemble evidence from several contemporary groups, geographically close if we are fortunate, or at least close enough for possible contact. Material which does not fall into groups but can be assigned to the same period or generation with reasonable certainty may be drawn on for further comparison. Finally, insofar as the archaeological data allow, we can establish sequences of groups, in time and in place, and demonstrate the course of stylistic change.

While we are undertaking our reconstruction of visual environment, and our stylistic analysis of groups, we will also be considering the larger setting, that is, the society within which the artist works, since this too will affect, immediately, his choices and the functions of his creations, as well as the character of his visual setting. Here we can use the valuable study of Renfrew, who has organized the archaeological evidence for the EBA along engineering lines, in a systems analysis (Renfrew 1972). Without adopting all of his terms and conclusions, I propose to use his convenient division of culture into five areas: subsistence, technology, trade, social and political systems, and a last category which we may call symbolic.[3] Each of these areas, as Renfrew demonstrates at length, affects each of the others in many ways, some clearly apparent, some only inferred, and consequently each has an effect on the arts.

Thus, *subsistence*, in our period, is based in part on two new crops, olive and grape, which will affect the framework of daily life by changing patterns of cultivation and settlement, and by requiring new methods of processing and storage, and the production of new vessels suitable for oil and wine (Renfrew 1972: 265-307, especially 281-85, 291; J. M. Renfrew 1973: 127, 132).

Innovations in *technology* are chiefly in metallurgy, but the influence of metal forms has long been suspected in pottery, and the new metal tools may well have changed the arts of carving in soft materials (Renfrew 1972: 308-38). New techniques seem to have been introduced also in stonework. The tubular drill and the potter's wheel

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[2] It has sometimes been doubted whether "universally valid criteria" exist which would entitle us to make formal analyses of an art so far removed from us. But studies of the psychology of vision (e.g. Arnheim 1974), and observations by anthropologists of primitive artists at work suggest that the human eye does perceive principles of balance, symmetry, color and the like in predictable ways. See e.g. Firth 1966: 13; Goodale and Koss 1971: 195.

[3] I use this term rather than Renfrew's "symbolic-projective" in the interests of simplicity. The systems (or sub-systems) are discussed in Renfrew 1972: 225-504.

supplied a new impetus at the end of the EBA, if not sooner in some areas (Warren 1969; Renfrew 1972: 346-48).

Trade is a large subject, and in many ways an unknown factor. The clear evidence of the extraordinary obsidian traffic warns us against underestimating the importance of trade (Renfrew 1972: 442-43), but its exact nature eludes us for the most part. We can document contacts within the Aegean, both through direct imports and through notable imitations, We can also propose the existence of wider contacts, especially with Egypt; certainly these contacts must have been regular by the end of our period to account for the presence of scarabs in EM III-MM IA contexts.[4] The undoubted fact that writing, and seals, were in use both in Egypt and in Mesopotamia from at least the beginning of the third millennium seems to me of great potential significance. But the mechanics of transfer, both of objects and of ideas, are too little understood.[5]

The evidence which Renfrew presents for a notable change in the organization of *society* during the third millennium seems to me compelling, at least in outline (Renfrew 1972: 362-403, especially 371-83, 392-99, 491-92). He demonstrates an unequal distribution of wealth in the Cycladic graves, and the centralization and consolidation of power through the presence of central buildings and fortifications at Troy, Lerna, and other sites. Life is undoubtedly more complicated in the third millennium than it was in the fourth, at least in the social sense, and this fact has consequences for the maker of objects. Walls and buildings must be designed and constructed, suitable objects of valuable materials must be made to show the importance of the persons whose authority is most effective in the community, and thus to establish the permanence and value of the community itself. Specific marks of ownership are now needed; hence the seals. Not everyone will have the skill to produce the objects, just as not everyone is entitled to hold the authority of chief or prince and, as the specialization increases, the work becomes increasingly more skillful. That is not to say that there were no specialists in the Neolithic age, or that an abrupt transiton is made during EB II to an organization of full-time specialists. But widely varying degrees of skill certainly do exist now and have to be taken into account (Renfrew 1972: 339-61, 491-92).

The last of the areas, the *symbolic*, in which the visual arts have their place, is perhaps the most difficult, and the most interesting. Here Renfrew includes all expressions of religion, writing, principles of measurement, regalia, and all the arts:

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[4] E. g. in the Arkhanes osteotheke, *CMS*, II, no. 395.

[5] New approaches to the subject of trade can be found in Renfrew 1972: 440-75, with references; 1975; Earl and Ericson 1977.

anything through which man interprets the world to himself. He quotes Cassirer: "... Man lives in a symbolic universe ... No longer can man confront reality immediately; he cannot see, as it were, face to face." (Renfrew 1972: 405; Cassirer 1944: 24-25). Cassirer's description is true to some degree of man at any period of his existence, in any social setting, even the Palaeolithic. But when one considers the Neolithic symbolic forms and contrasts them with those of the Middle Bronze Age, specifically the Middle Minoan, one sees immediately that enormous changes must have taken place during the EBA (Renfrew 1972: 404-39).

In particular there seems to be, in this area of symbol and art, one definitive change which emerges during the EBA. We may perhaps describe it as the concept of *equivalence*, if I may so expand Renfrew's use of the term. He suggests that the use of seals may signify the beginning of a potential equivalency of goods and services, an economic development which was far from complete even in Mycenaean times and indeed does not reach a full complexity until the invention of coined money (Ventris and Chadwick 1973: 198; Polanyi 1960: 329). In this philosophic and economic sense, equivalence can also be seen both in the use of seals as signs of ownership and in the variety of marks on pottery as we see them at a number of sites.[6] These are certainly preliminary stages in the development of a written language, the achievement of the early palatial period. We can also apply the concept of equivalence to the invention of units of measure, both of weight and of length. Renfrew argues persuasively for the existence of these in the third millenium (Renfrew 1972: 408-11). It seems certainly probable that, as in the case of writing, some at least preliminary stage of mensuration existed before the first palace courts were laid out in MM I: if not actual units in every case, then at least the concept of ratios, as Getz-Preziosi has demonstrated for the Cycladic figurines.[7] The symbolic value of regalia and various forms of conspicuous wealth, too, may be treated as a form of equivalence; it seems clear that these are new phenomena in the EBA (Renfrew 1972: 370-83).

On the assumption that all this material evidence has been properly interpreted by Renfrew and others, it may be possible to connect it further with another new development in the EBA, that of representational art. One should acknowledge of course that representational art exists from a much earlier period, and the use of symbol from at least the beginnings of language. But during the third millennium

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[6] As at Phylakopi (Renfrew 1972: 512) and Lerna (Caskey 1956: pl. 45f; and others to be published by W. McLeod).

[7] Getz-Preziosi 1977. Graham expresses doubt as to the use of a standard foot at Lerna (personal communication): see Caskey 1955: 40. Some form of orderly abstraction, however, must surely lie behind the plan of the House of the Tiles.

we encounter many more examples of representation; there appears to be a new consciousness of physical forms, a new channel of thought by which the mind and hands of the artist are led to model and then to draw animals, human beings, ships, houses, and the like in forms that our eyes can read. This power of mental transformation seems to me analogous to the new equivalence in measurement and in economics which we have just been noting; it is the ability to think of one thing in terms of another essentially unrelated thing. A building becomes a series of abstract units or ratios; or a man's wheat crop becomes a mark on clay which in turn becomes a promise of defense or a bronze tool; a pot becomes a boat or a bird, or a lump of gold a flower.

It may be helpful here to look briefly at some anthropological interpretations of visual symbol among modern primitive peoples. Forge, in a study of Sepik painting in New Guinea, shows that our assumption of "an absolute translatability between verbal and visual system" (Forge 1973: 187) does not hold among the Sepik people: a form which to our eyes appears vaguely humanoid may be called "a man", but so may another, roughly similar, which seems to have no identifiably human features at all. The circles which are called "stars" may equally well be called "eyes" by another informant or on another day. Boas noted a similar variability of interpretation in American Indian myth and visual art: the same conventional motifs might be assigned different totemic meanings by members of different tribes to explain widely different natural phenomena or social customs (Boas 1955: 108, 128). It may be reasonable to suppose that such variability obtained in Neolithic Greece, and that on the contrary it did not for the most part in the Middle Minoan period, when both writing and representative art come into use.[8] This difference is not to be seen as a process of steady and complete change to a state of "absolute translatability", but only to an eventual working assumption that such an idea can be valid. In our own time, we assume in general that our economic life, and our spiritual life as well, is based on fixed equivalences of goods and services, so that we can confidently state the value of a housewife or of a college education in dollars and cents; and we are commonly inclined to think that our art could be interpreted with equal confidence by our artists if they were not so reluctant to say what they mean in clear words.

It would seem therefore, in a very general sense, that we persist in holding to an idea of equivalency of fixed values and fixed meanings, while primitive peoples on

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[8] I say "for the most part"; Olivier (1981) has suggested the possible difficulties in determining which Cretan hieroglyphic inscriptions are to be "read" in an ordinary modern sense when the forms of the signs and their arrangements are often so highly decorative, or when certain signs cannot be definitely identified as belonging to the writing system, at least as it is known on clay.

the whole do not. This equivalence is the idea which appears to be new in the pre-urban stage of culture we know as the EBA, not an idea which appears overnight or which supplants the older ideas entirely, but one which has eventually a profound transforming effect on art and on life.

Let me make one further point. The new interest in representation as equivalence is only one of many elements which make up EBA art. This art is to be seen as a whole as a process of change, not, as in the older biological manner, as a blossoming or growth leading ultimately to decay and arising from immaturity.[9] The danger in that view is that we may regard the early products of Bronze Age artists as works of an immature mentality like that of children, and the latest Mycenaean works as somehow morally deficient or senile. It should hardly need to be said that adults, in any age, are not children. There are always incompetent or second-rate artists (that is another problem), and one of our tasks will be to identify and study especially the work of the best—and to contrast it with the average and the careless.

In order to try out these proposed axioms and principles, I should like to look in some detail at three groups of EBA material, and then to return to some further consideration of representational art.

Let me begin, appropriately, with Caskey's own site of Lerna.[10] We know it as a long settled and well watered site on the low and fertile Argive coast. We have three classes of major evidence for the art of Lerna in the last stage of EH II: the House of the Tiles itself, the pottery in it, and the seal impressions.[11] If we take the three and look for common ground among them, we may find it in principles of form and syntax, expressive of the artists' choices and the functions of the objects created.

For the House (Pl. 11), the full range of domestic pottery found inside together with seal impressions on clay, bronze tools, an obsidian core (Room V) and the mass of lead in Room XII, as well as the complex system of entrances, suggest a good deal of practical as well as supervisory and economic use. The eastern entrance allows a direct approach from the sea gate in the fortifications to the lower floor, as does the less important western one. The second floor is reached directly through Room III without passing through the ground floor at all, or through the internal stair; the offices Room I and Room XI are available for purely external business.

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[9] Cf. Ridgway 1977: 5; and Vermeule 1968: 92-93 especially, who express such warnings eloquently for other periods of Greek art.

[10] I thank Professor Caskey for his permission to present both published and unpublished material from Lerna. For reports on the excavation seasons at Lerna see Caskey 1954-Caskey 1959.

[11] "A variety of tools" can also be cited; some of these, including perhaps a small crystal bead from Room IV, may be from bothroi of EH III dug into the House of the Tiles debris. See Banks 1970: 238, bead no. 440.

If the presence of part of a hearth in the North Corridor IV is analogous to the place of the hearth in Building BG (Caskey 1958; 130, fig. 1; 1959: 203), we may have evidence of a cult room as well. The House is clearly no private mansion; it is designed for a range of uses. Given these uses, and given also certain traditions, the architect made his choices. The compact, closed, rectangular form is set in its own space, easily comprehended by the eye, its complexities contained within its firm boundaries. And so the seals: they are bounded and compact, focussed inward rather than outward, their precise outlines at times further defined with a line border (*CMS*, V, nos. 55 etc.). One need only think for contrast of the Minoan palace plan, growing outward from its central court, or of the Cretan seals with characteristic rapport design which defies the outline of the face (e.g. *CMS*, II, no. 60). The management of the interior space in the House has also something in common with that of the seals: a balancing of variously sized spaces, a desire for variety closely controlled by principles of symmetry. The rectangular form, the balancing corridors along two opposite sides, creating a modified axial symmetry, the alternation of large and small rooms, all seem to be in the tradition as we see it in Building BG, and at Akovitika.[12] Doorways set off center in the wall are usual in EH II domestic architecture: here at Lerna this asymmetry is countered by the alignment of a series of these doorways within the House.[13] The exterior doorways are more symmetrically set within the facades. The use of a uniform width for walls and doorways further consolidates the variety within the House.[14]

Similarly in the seals, there is a strong sense of axial symmetry (*CMS*, V, nos. 65, 66, 76, 80, 81, 87, 88) which does not preclude occasional non-symmetrical variation (*CMS*, V, nos. 57 (Pl. 12a), 58, 78, 97), a balancing of large and small forms (nos. 56 (Pl. 12b), 61-64, 72, 92) and of a series of asymmetrical parts in a symmetrical manner (*CMS*, V, nos. 77 (Pl. 12c), 110 (Pl. 12d)). But there is never any breaking of the containing shell, and one always knows where one is, as indeed one does in the House. If we understood better the practical demands met by these objects, we could say perhaps how much is the fruit of individual inspiration. The placement of the two external offices, or the internal access to the North Corridor IV, which determine much of the plan of the House, may be a response to the local economic or religious dictator; or the small motifs on the seals may be perhaps a

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[12] Building BG, Caskey 1959: 203, fig. 1. Akovitika, Karageorgha 1971: 126-129, and a predecessor to the House of the Pithoi (Pullen 1985). Possibly also Zygouries, House Y, Blegen 1928 (2): 25, fig. 21.

[13] Similarly aligned as Askitario, Theochares 1954 (2): 63, fig. 4, Houses A and E; and at Ayios Kosmas, Mylonas 1959: fig. 1, Houses E and F. Cf. Shaw 1985.

[14] See n. 7.

totemic requirement by members of social groups. But the total organization of these half-seen but clearly competing demands and traditions into a coherent and visually satisfying whole must be the work of individual minds of a high order.

The pottery, the latest of the Lerna III pottery groups, is less interesting but presents some points of comparison. It is exceedingly competent, as is nearly all Lerna III pottery (Pl. 12e, f). Practical functions are satisfied by a range of about a dozen basic shapes. The subtle fluid curves of the shapes can still be seen (Caskey 1960: 290, fig. 1, no. IV) and compared with the curves in seal designs, but they are not so remarkable as in the earlier groups from Rooms CA and DM,[15] or at other sites, as for example at Askitario. Here in this thickly settled fortified town on the Attic coast, a master potter, or his workshop, made a series of sauceboats and jugs which show an extreme elegance of form and polished surface: one sauceboat (Pl. 13a) for example with extraordinarily elongated spout and forward tapering ears, a low base, and slanting horizontal handle;[16] and from the same house a beaked jug (Pl. 13b) with its perfect curves and a herringbone handle in tactile contrast to the smooth polished gray surfaces, set off by dark paint on the lower body.[17]

A few fragments of particularly fine ware from the House of the Tiles suggest that work of similarly painstaking design and finish did exist there once: a sauceboat sherd from Room XI, the sealing office, with a polished surface and handsome flaring ribbed handle (L. 1557) (Pl. 13c): or one from Room I, also finished in lustrous browns (L. 1559). It is not unreasonable to think that there were also workers in metal, here or nearby, who could produce like things in bronze or gold. Decoration on pottery is rare. It is plastic, tactile more often than graphic, and emphasizes the crucial transitions at rim and belly, as does the wood that frames doorways in the House. But two sherds from the western Room V remind us that considerable graphic skills did exist, although we no longer know to what purposes they were ordinarily put—perhaps painting on perishable surfaces? The sherds (L.191, 192) (Pl. 13d-g) come probably both from the same saucer, of ordinary Lerna III type with inturned and painted rim. The decoration, if it is that, is on both sides and in at least three colors. On the exterior of the small sherd is an X and four dots in white, with an additional red dot. On the larger sherd there are larger circles, one dark, one creamy white, with superimposed dots and strokes in white and red or black which give almost the effect of human faces with rudimentary bodies attached. On the interior is a broad dark band on the small sherd with superimposed white dots,

[15] Caskey 1956: pl. 46 except *e* from Room DM; 1960: 290, fig. 1, no. IV.

[16] Illustrated in Matz 1962: pl. 6, NM 8858.

[17] Theochares 1954 (2): 70, fig. 11; 1955: pl. 34β, NM 8863. I owe the late Professor Theochares many thanks for his permission to study and photograph material from Askitario.

while on the larger is another dark band with dot, and another series of circles in two rows, each marked with an X. These Xs are done in alternate strokes, all the upper left to lower right in creamy white done first, the crossing strokes after in a brighter and more fugitive white, all done by a hand clearly practised at using the brush. They may be tally marks, like the incised pot marks, or representations, but certainly they are done intentionally and with skill.

I should like to turn now away from the mainland to a contemporary site on Crete. To the far from clear picture of prepalatial Crete, Fournou Korifi (Pl. 14) near Myrtos on the South East coast has contributed a wealth of lucid detail. When it was excavated by Warren in the nineteen sixties it was the first EM II settlement site to be fully investigated since Seager worked at Vasiliki early in the century.[18]

The Myrtos settlement of perhaps a hundred persons, on the top of a steep, windy, hot hill overlooking the Libyan Sea, lasted for several centuries, according to its excavator, until its destruction about 2170 B.C. (Warren 1972 (1): 271). There may have been a similar settlement on the nearby hill of Pyrgos, and indeed many such during the second EBA in the East of Crete (Cadogan 1978 (1): 70-71). At the time of the destruction of Fournou Korifi, there were about eighty rooms in use, and over six hundred pots in them, which give us a good basis for considering functions and choices. The plan, if that is the word for it, presents us with an agglomeration of roughly rectangular rooms, not of individual free-standing houses, but of clusters, separated by occupation; wool-working, weaving, pottery-making, shrine and cult rooms, storage, kitchens and workrooms. Some of the rooms may be lost to the east, perhaps also to the west, but we have the angled south facade, with a raised way along the outside, and a jogged entrance passage. As Warren says, Myrtos and Vasiliki give us predecessors in some sense for the compact later Minoan towns like Gournia, and for the palaces themselves (Warren 1972 (1): 260-61). Only the central court appears to be absent. Perhaps the open areas Warren points out (nos. 30, 45, 69) provided a similar central focus. Or perhaps we are mesmerized by the palatial courts which are rather the inspiration of a Middle Minoan Daedalus, while the original impetus for the palaces lies in the occupational groupings, something like Evans's blocks of insulae proposed for the earliest palace at Knossos. Whatever the nature of the Myrtos plan and its origin, it is not haphazard: it conforms to the contours of the hill, and it is highly organized, like the palaces, from an internal, not an external, point of view. Within this hive of small dark red-plastered rooms, terraces, trapdoors and ladders, a quantity of useful goods was being produced, not

[18] I thank Professor Warren for allowing me to examine and photograph pottery from his excavation; Warren 1972 (1). For a new interpretation of the settlement see Whitelaw 1979.

probably entirely for consumption by the community. We can only imagine the woolen materials; but we have the pottery, some of it perhaps brought in from Vasiliki, much of it probably made here. The shapes are much more various than in EM I; so are the fabrics. We may take for example typical jugs of the Myrtos ware (Warren 1972 (1): pl. 52, P443 (Pl. 15a), P445), with long striped triangles in dark paint and horizontally striped short necks; here we see already the rather elongated high-shouldered shape that characterizes later Cretan pots. There is a deliberate balance of ornament against empty ground which enhances the competent elegance of the shape but does not directly repeat it. We are made aware of a play of tension between decoration and shape; in some cases we see also experimentation with decoration (Warren 1972 (1): pl. 52, P460). Here the red patterns suggest pieces of fabric, with a seam down the front and a diamond at the back.

The pleasure in mastering a blank space—graffiti-pleasure perhaps we should call it—is obvious in the large vessels, storage pithoi or amphorae, or piriform jars, which are decorated with trickle ornament (Warren 1972 (1): pls. 59-61). There is always an abundance of empty ground, although the entire neck above a band of impressed ornament may be painted (Warren 1972 (1): pl. 59, P608) (Pl. 15c). One unique hydria (Warren 1972 (1): pl. 48, P392) (Pl. 15d) shows two ribs at the neck, not, characteristically, at the exact point of junction between the neck and the body, but above and below. The pot shows us a sense of play with form, a desire not to ignore the functional transitions, but to call attention to them in more subtle ways than by underlining, to regard them as fluid, and to explore surfaces and volumes.

Some such impulse may lie behind the Vasiliki ware decoration as well. Warren has shown conclusively that the variety of colors occurs in deliberately controlled patterns, accomplished, it seems, by the application of varying thicknesses of paint and firing in successively reducing and oxidizing atmospheres (Warren 1972 (1): 129-30; e.g. pl. 49, P401, (Pl. 15e), P403). Perhaps it is the stone ware made for contemporary graves which inspired the patterns originally,[19] but the burnished paint has its own effects. The structural features are not stressed; there is no rib at the base of the neck; the handle is small; only the base is set off with a ring. The exploitation of the surface of the vase is all, and while symmetry is important the elements do not echo the shape but lead the eye restlessly around it. We have here in fact the beginning of Matz's principle of torsion (Matz 1928: 156, 161).

There remain the seals. There are four finished and two unfinished stone seals from Myrtos, and one clay sealing found by a doorway (*CMS*, V, nos. 14-19, sealing no. 20 (Pl. 15b); Warren 1972 (1): 226-27). We can detect a certain skill in shaping

[19] E.g. Mochlos: Seager 1912: col. pls. 1, 4-6.

the stones, none to speak of in what we might call the "cut and slash" decoration on the faces.[20] One wonders how much actual sealing was done with these objects. But there is no doubt that sealing took place. The design of the impression is certainly, for all of its poor preservation, a deliberately organized affair, with strict attention to the outline of the seal face, to symmetry, and to the use of the space created within the design. We have to look elsewhere, however, for any indication that seal cutting in EM II approached the complexity and restless spatial consciousness that we saw in the architecture and pottery of Myrtos: to the seals from the lower levels of tomb IIA at Lebena and tomb E at Arkhanes.[21]

Let us look now at another roughly contemporary and well-known group of finds, this one from the Cyclades. Settlements are not well-known here, and tomb groups offer us the best materials for stylistic analysis, although there is as yet not much consensus on the sequence or relationships of Cycladic cultures.[22] The contents of Tomb 10 from the Spedos cemetery on the inhospitable South East coast of Naxos are generally assigned to the Syros period, that is EB II, and are displayed as a group in the National Museum in Athens.[23] It is an unusually rich group for a Cycladic tomb, and rich for our purposes, with a dozen objects of both stone and clay, including two Folded Arm Figurines, of the type called "Developed" by Getz-Preziosi (or "Spedos" by Renfrew) (Getz-Preziosi and Weinberg 1970: 5, fig. 1d; Renfrew 1972: 423, fig. 19, 6, IV F). The figurines are remarkably large, one over 50 cm., rather long and elegant in their relatively natural proportions. The larger one (Papathanasopoulos 1962: pl. 40α; Zervos 1957: no. 113) is especially carefully made. The articulation of all significant transitions by straight grooves (fingers and toes, neck-head, body-neck, belly-pubic triangle, arms-body, spine, and the separation of the legs formed by another exaggerated groove), contrast with the carefully varied and subtle alternating curves of the outline (only the neck is straight in outline). Yet though it is thin seen in side view it is not a two-dimensional piece; belly, thighs and breasts are carefully rounded. Getz-Preziosi's suggestion of a

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[20] Similar decoration occurs on the Lebena Tomb II (lower level) and Tomb IIA (lower level) seals, dated respectively EM I and EM II: *CMS*, II, nos. 195-97, 214. Hatching also occurs at Lebena: *CMS*, II, nos. 199, 210-13, 215-16; and indeed, in a more regular form, continues to be found among the Phaistos seal impressions (*CMS*, II, nos. 1-11) dated to Classical Kamares by Walberg (1978).

[21] Lebena Tomb II: *CMS*, II, nos. 213 and 216. Arkhanes Tomb E: Sakellarakis 1975 (2): 295, 304, pl. 245β; 1975 (2): 163-70.

[22] The study by Doumas (1977) and the chapters by Renfrew (1972; further discussions in Renfrew 1977 (1): 17-30) give two recent analyses which do not agree in all points. See also Coleman 1974: 333-43.

[23] Excavated largely in 1903 by K. Stephanos, but the group was reassembled from his meagre notes and published by Papathanasopoulos (1962: 114-132).

canon of proportions used by figurine sculptors (Getz-Preziosi 1977: 71-91) confirms this impression of deliberate care. The second figurine (Papathanasopoulos 1962: pl. 40β; Zervos 1957: no. 115) repeats the traditional grooving, and attempts the modeling, but the subtle curve of waist to thigh is lost, and the exaggerated size of head and neck has contributed to the meagerness of the torso and arms.[24]

The marble vessels have not the uncompromisingly severe line of earlier Cycladic ones. As in the figurines, there is more articulation of functional parts: flat or hollowed bottoms on the two shallow bowls, a definitive spout and lug on the third deeper one (Papathanasopoulos 1962: pl. 50α).[25] The two fine large clay "δοχεῖα" (Papathanasopoulos 1962: pl. 50β, γ) show the same flaring conical foot, and a gentle transition from globular lower body to concave upper one and well defined projecting rim. The bands of carefully incised diagonal hatching here emphasize the rim and the transition between the curves; but the handles are set lower and closer to the maximum diameter. These forms are related to the early marble Kandili ones (e.g. Renfrew 1972: pl. 1, 3), but in the Kandili the transition from neck to body is most abrupt, and the upper and lower conical parts of the vase more nearly straight.

Finally, the four painted vases from the tomb show again rather complex forms, and a fondness for rectilinear surface decoration (Papathanasopoulos 1962: sauceboat, pl. 49β, col. pl. B; large jug, pl. 49γ, col. pl. G; small jug, pl. 49δ; triple sauceboat, pl. 49α, col. pl. A) as on the two δοχεῖα and figurines, in contrast to the complex curves of the shapes. On the sauceboat, the lower mass of the wide low body and the reverse curve of the graceful spout are painted in a solid red, changing to black at the top beneath the spout. The rim, inside and out, bears hatched pendent triangles, further elaborated on the outside with a band of herringbone, where the width of the decoration decreases with the diminution of the spout. The base is articulated with a ring. The two jugs, the larger incomplete but presumably also round-bodied, are covered with a net of pattern: the same pendent triangles at the rim (on the larger jug rolled into a tube to make a spout), necks set off with stripes

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[24] Getz-Preziosi (1966: 106, n. 6) thinks these two figurines are by the same artist. Papathanasopoulos (1962: 115) suggests not the same artist, but the same workshop; I am inclined to agree with the latter.

[25] Cf. Papathanasopoulos 1962: pl. 48, NM 6134. If this vessel is a lamp, as Stephanos (1905: 216) and Renfrew (1972: 285) suggested, it is a model, not a working lamp, since it is solid marble and neither mouth nor spouts are pierced. A workable pierced example with five spouts, of dark gray polished clay, can be seen in the Naxos Museum in the Koufonisi case. The presence of oil in the small painted jug (Stephanos and Renfrew) does not guarantee that both the marble spouted vessel and the triple sauceboat, which does not show signs of burning, were intended, even ceremonially, as lamps (Renfrew 1972: 269), but it is one possible interpretation.

or bands of herringbone and hatching, handles decorated with hatching, and bodies with broad triangles of hatching, alternating here with filled lozenges in vertical rows. The effect is exuberant, not to say extravagant, when one considers also the twisted handle and the careful plastic buttons at its top, but it is not uncontrolled; and the variety of curves in the outline, particularly in the generous swing of the handle, and the unusual precision of the decoration, show an able and bold maker of pots. His ability extended to improvisation too in the triple sauceboat. The same low graceful shape appears that we saw before, with similar rim decoration, but plain bodies, joined at the rear, and the horizontal handles for which there was no room transferred to the spouts. The conical base repeats the ornament of the inner rims; but the three long stems are defined by a different motif: horizontal lozenges strung together like beads. Conjoined or multiple vases had a function in tombs,[26] but this one must have been remarkable even in its day. The stems may not be the happiest of inspirations, but they are bravely done.

Further evidence towards our understanding of this group may come perhaps from the smaller painted jug, similar in decoration and technique to the other painted pots, especially to the incomplete jug. It may have had a coating of silver, despite its decoration, and it contained oil.[27] To learn more, we should have to consider other Cycladic groups, and here we are handicapped, as in Crete, by the lack of stratified evidence. We do not know precisely either the geographical extent or the chronological durations of the cultural groups which have been named Pelos, Syros, Kastri, Amorgos, Phylakopi I, nor are we quite certain about their places in relation to the various Early Helladic and Early Minoan subdivisions.[28] But a careful stylistic study of grave groups, and of EBA II and III groups on the mainland and in Crete, should help to define some of the differences more accurately. The expansively rich collection in Spedos Tomb 10 ought to be one of the keys, particularly if we regard the decorated pottery as the work of a single potter.

Painted decoration is known from Tsountas's Chalandriani tombs on Syros, and from certain other Cycladic sites, as well as on the mainland,[29] but it has never been

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[26] Kernoi, as for example in the Mesara tholoi (Xanthoudides 1924: (Koumasa) pls. 24, no. 677, 25, no. 4194; and many from Platanos) and at Phylakopi: see n. 34.

[27] See n. 25. Papathanasopoulos 1962: 118. There are dark patches adhering to neck and shoulder.

[28] See n. 22 for further bibliography; see also Davis and Cherry 1979.

[29] Tsountas 1899. Naxos: Papathanasopoulos 1962: pl. 56, 72; and other examples in Naxos and Apeiranthos Museums. Keros: Renfrew 1972: 531-32. Keos, Ayia Irini: Caskey 1972: pl. 77, B12-B15. Cycladic examples illustrated in Zervos 1957. Lerna: Caskey 1960: 292, pl. 69b, c. Askitario: Theochares 1954 (2): 67, fig. 6, a jug said to be late EH II in date, that is, earlier in context than the elegant pots from House E described above. Rafina: Theochares 1951: 84, figs. 8β, 9.

fully studied and published, nor are all of its associations entirely clear.[30] The Keos sauceboat sherds were found in firm EBA II context and include the strung or pendant lozenges which we see on many pots of the Keros-Syros class,[31] and in particular (in the strung version) on the Spedos Tomb 10 triple sauceboat stand. Decorated sauceboat sherds at Lerna were found with some of the earliest Lerna III material including *Urfirnis* of good quality, and some resemble the Naxos sauceboat closely in quality of fabric and paint. The low broad-spouted shape of the Spedos sauceboat is also paralleled among early Lerna III pieces.[32] One sauceboat rim from Ayios Kosmas is also a close parallel with its pendent triangles and herringbone decoration (Mylonas 1959: fig. 160, no. 312). On the other hand, the spouted bowl and the marble triple-spouted vessel seem to point to the spouted shapes which belong with the Kastri group,[33] although they are not precisely similar, while the multiple sauceboat should be a forerunner of the well-known Phylakopi I kernoi.[34] It may well be that a close study of the Keros-Syros pottery and the current reexamination of the Phylakopi site by the British School at Athens may be able to establish both the continuity and the discontinuities of the later style, and the relationship between

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[30] It should be noted that the fine surface and fairly thin walls of the painted ware form a contrast to the majority of Keros-Syros pottery. Tsountas (1899: 98) took pains to point out that examples were found in three Chalandriani tombs together with frying pans, and that the ware appeared to be contemporary with the rest of the material, although he suggested that it might have been produced elsewhere. A one-handled hatched cup was found in tomb 142 on Siphnos (Tsountas 1899: 75-77, pl. 8, 3) with four of the one-handled tankards which Rutter (1979) associates with the Kastri or Lefkandi I group. The baggy shape of the cup resembles that of the individual vessels which form the Phylakopi I kernoi (e.g. Zervos 1957: 149). The associations of the other painted pots at Chalandriani are not perfectly certain. The frying pans from Tombs 236, 371, and 408 are not illustrated by Tsountas, but one displayed as from Tomb 236 in the National Museum (NM 5012) does not have the canonical Syros form. Other decorated pots from Tombs 166, 186, and 371 are said to have been found with "black" vessels: two pyxides and a jug (Tsountas 1899: 109-11); one wonders if these were black-polished pots of the Kastri group. But on Naxos a patterned jug was found in a tomb at Phyrroges together with a Folded Arm Figurine (Papathanasopoulos 1962: pl. 72b), and another similar jug in Munich (Thimme 1977: 586-88, no. 416) is said to form part of a grave group which includes a Folded Arm Figurine of Spedos type.

[31] Dotted rhomboids in paint are often used to represent the eyes of the figurines: see Preziosi and Weinberg 1970: 8, fig. 5, pls. 3, 4, 2, 3, and 5; and indeed the Keos and Syros teddy bears. The Spedos Tomb 10 figurine (Zervos 1957: no. 113; NM 6195) retains traces of black paint on the face (Papathanasopoulos 1962: 114) and we have no reason to suppose it is not contemporary with the pottery.

[32] Lerna sherds from lots J98, A47, HTJ 26, HTJ 25. Sauceboat profile illustrated in Caskey 1960: 290, fig. 1.I.

[33] E.g. Tsountas 1899: pl. 9.14 (Tomb 392, found with a one-handled tankard). See n. 30.

[34] Kernoi: e.g. Zervos 1957: nos. 145-151. Sauceboats in the Cyclades are widely diverse in character and occur as late as Renfrew's Amorgos group (Renfrew 1972: 534-35; Tsountas 1898: pl. 9,7).

both Cycladic groups and the EH III (Renfrew's Tiryns culture) painted ware of the mainland.[35] Clearly at Spedos a vigorous artist was working in an established EBA II tradition in company with artists skilled in stone carving, and for a clientele who expected or deserved exceptional work. Such stimuli would have encouraged the production of remarkable objects, the exploitation of the decorative repertoire, and a reaching for new forms of visual expression and the perfection of the old.

The Spedos Tomb 10 group seems to sit in the center of a web of complex parallels. We are looking here into the beginnings of a period of complicated and intense change, in which the broadly similar societies of the second EBA are fragmented into groups whose relationships are not yet clear. On Crete the change seems little disruptive of society, but on the mainland it is deep and lasting.

One of the clues to this continuity or divergence lies in the concept of equivalence. A new interest in representative art characterizes the second EBA, and on Crete the succeeding phases as well. If we treat representative forms as a variety of equivalence, it may be helpful now to review some of the evidence for visual representation, and to see where it takes us.

We have just considered one of the most characteristic examples and certainly the best known type: the Cycladic Folded Arm Figurine, in one context on the island of Naxos. Surely Renfrew must be right in postulating some chain of descent from the Neolithic ladies on through the figurines of the third millennium (Renfrew 1972: 420-21; 1969: 1-38; and (modified) 1977: 59-71; Getz-Preziosi and Weinberg 1970: 4). But the descent does not explain the form except in a most general sense. We are still faced with questions of choice and function. Why were these figurines made, and why do they look the way they do? The function we still know little about. Obviously the figurines were useful to the dead, because they are found in graves, but they seem to have been valuable to the living, too.[36] As for the choices which produce the form, some are determined by the artist's local tradition, some by the long temporal tradition, artist learning from artist,[37] some by the availability of fine marble, which was to be responsible, much later, for the early development of marble *kouroi* in the Cyclades (Ridgway 1977: 38, 46). But others of these choices are the same ones of proportion, articulation, and mass which are being made by contemporary potters and metalworkers, and which may be controlled by the new canons and concepts of measurement.

And what do the artists mean by these figurines? I have suggested above that the Neolithic peoples may not have regarded their figurines as possessing a single

[35] See Caskey 1971: 788; 1960: 295-97; Rutter 1979.
[36] Renfrew 1972: 424 on finds in settlements.
[37] Cf. the major thesis in Gombrich 1960.

readable significance but rather a range of meanings which varies according to place and circumstance. Similarly, the Cycladic figurines may have continued to have been "read" as generally female in a variety of contexts (as mother goddess, protector of fertility, of children, of water or towns, guide to the dead, etc.)[38] and the coexistent schematic figurines and frying pans (Coleman 1977: 113-14; Tsountas 1899: 89-92; Zschietzschmann 1935: 658) perceived as equally satisfactory renderings of the same or of yet other ideas. Concepts which we might even regard as mutually exclusive may be embraced in one traditional image. But the seated figurines, the musicians, the cup-bearer in the Goulandris collection,[39] introduce, I think, another and newer idea. These are surely to be read just as they appear to us: as people engaged in human occupations. Somehow, in this period, the artist has achieved enough detachment from the lives of others to create a form equivalent to a human action in time. His own role is no longer interchangeable with others as it was in an earlier more egalitarian society, but specialized; he is set apart and forced to see facets of life from a certain distance. So definitive a change may belong fairly late in the sequence of EBA art, but there are signs of its stirring throughout the third millennium.

The human urge to create symbols, and specifically visual images, is given great impetus by the proliferation of pottery forms in the EBA. Pots, in particular, lend themselves to anthropomorphism (or perhaps zoomorphism); we do it ourselves in our verbal descriptions: pots have bodies, feet, handles, necks, sometimes ears, or beaks, or wings. The sauceboat is irresistible; sometimes it borrows a ram's head from contemporary figurines (Blegen 1928 (2): pls. 11, no. 251; 10, 2, no. 260). As early as the Lebena II tholos, and in many examples from Koumasa, there are pots which have developed bird or animal heads.[40] Pyxides lend themselves easily to the house shape, in stone as well as in clay.[41] The coarse three- or four-legged pot stand is turned into a sort of elephant at Ayios Kosmas (Mylonas 1959: figs. 171-72), and pots which have become human look at us from Troy and Keos, and Basle.[42] Much of this is surely play, but play with the medium is serious business among artists.

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[38] Thimme 1965: 72, and Schefold 1965: 87-90, discuss a number of possible interpretations.

[39] Doumas 1968: 104, no. 142. Also a fragment of an embracing group, from Aplomata Tomb XXVII on Naxos: Lambrinoudakis 1976: 155, fig. 133.

[40] Lebena: Alexiou 1960: 225-27, figs. 9, 10, 14. Koumasa: Xanthoudides 1924: pls. 20. 4141, 4142, pls. 28, 4146, 4121-4124. A jug in the Apeiranthos Museum, Naxos, is given dotted rhomboids for eyes on the sides of the spout. Cf. MC jugs, as in Zervos 1957: 281, 286.

[41] Thimme 1977: 336-37, nos. 360, 361. Compare Myrtos pyxis P642 (Warren 1972 (1): pl. 62), and Getz-Preziosi 1977: 102-104; figs. 85, 87, 88.

[42] Blegen *et al.*, 1950: Troy II shapes C30, D13. Keos, Ayia Irini: Caskey 1972: pl. 81, C51 a. Thimme 1977: 314, no. 281, and 97, fig. 78 in the Ashmolean Museum.

Whether it also demonstrates exact equivalence of meaning in every case is hard to say. It may be a play with equivalence, a deliberate ambiguity, surely in part for sheer pleasure. Exact equivalence seems more likely to appear, sooner, in non-human representations than in human; these are generally more easily recognizable, more "naturalistic", as we say. We need only contrast the Myrtos goddess (Warren 1972 (1): pls. 69-70) (Pl. 15f) with the teddy-bears from Keos and Syros.[43] Granted that the goddess is perhaps less than a masterpiece, it seems true that the concept "animal" is more easily rendered than the concept "woman". And a woman as goddess will carry further levels of unexpressed meaning at which we can scarcely guess, which can presumably be far better accommodated by this grotesque and barely human shape, the stalk of a neck and the bell body. Naturalism is here irrelevant, and to ask precise equivalence of form to meaning would be impertinent. On the other hand, the embracing arms and generous bodies of the bears are those of the Myrtos lady; they suit them, but project nothing of her numinous awe. At most we might be looking at creatures from a folk tale, or characters in the original age of Greek myth. Such animal representations seem to be freer, at times, even for unconventional experiment with form; witness the cow rhyton from House L at Eutresis (Goldman 1931: pl. 8), or the bull and vaulters from the Koumasa tholos (Xanthoudides 1924: pl. 28, no. 4126). There seems to be a similar directness in some of the rare graphic representations, like the dog on the Askitario pithos.[44] Here the dog is done clearly by a hand practised at drawing figures in fine curving outline, and informs us perhaps that the contents of the pithos are under guard.

This is a visual statement which we can read as clearly as we might the words *cave canem*. It seems to me that most of the graphic representations (and here I would include those on seals, and the ships on Cycladic frying pans)[45] are probably intended to carry such direct meanings. On Cretan seals, a plastic representation in the seal shape is earlier on the whole than any graphic representation on the face; none of the latter can be dated certainly as early as EM II.[46] In contrast to the mainland,

[43] Caskey 1972: pl. 77, Bl. Syros: Zervos 1957: 238-39. A third example, now in the Naxos Museum, from Panormos near Spedos, in Buchholz and Karageorghis 1973: 99, no. 1187. Another example, from Kheliotomylos near Corinth, is to be published by J. Lavezzi and was presented by him in a paper at the Fourth Annual Meeting of the Midwest Art History Society, 1977.

[44] Theochares 1954 (2): 72, fig. 19, from House E with the fine pottery mentioned above; see n. 22 and n. 23. Cf. the dog on the pithos bands from Lerna, Tiryns, and Zygouries (Wiencke 1970: 97, 103, S87: *CMS*, V, 120); on the silver diadem from Kastri, Syros (Caskey 1964: fig. 5); and on the gold diadem from Mochlos (Seager 1912: 29, fig. 9, II.4).

[45] Zervos 1957: 218-20, 223. Tsountas, 1899: 89-92. Graphic representations on seals: Lerna, *CMS*, V, nos. 57-59, 109, 110 if the striped objects are harps; and Keos, *CMS*, V, 2, nos. 464, 478. Cf. Coleman 1985: 198-99.

[46] Wiencke 1981; Sakellarakis 1981.

the Cretan development in seals as in pottery and architecture appears to continue unbroken into the palace period. It is thus in the Arkhanes osteotheke seals of the last Prepalatial period, certainly later than Lerna III, that we see the first major accomplishments of Cretan seal cutters, a wealth of imaginative forms, both plastic and graphic. The triple die may sum up the graphic accomplishments (*CMS*, II, no. 391). On several faces of this seal we see the border line, that useful device for limiting a field, as the Lerna artist had already discovered, used to define a picture space containing an animal, who is provided even with a ground line (side B) so that it may exist in its own world. This creation is a milestone on the way to later Cretan pictorial art, but the artist seems a shade wary of what he has done, and tries to summon up the decorative figure to which his eye is more accustomed by filling the empty picture space with small objects. We need only contrast the older decorative use of representational elements as on seals at Lerna, for example (e.g. *CMS*, V, no. 109). In these, the picture of the object, though not related pictorially to its setting, is in itself perfectly clear and was surely recognizable to the viewer. And the use of the seal to make recognizable marks which might substitute for the presence or the responsibility of the owner is simply another aspect of the same idea. But on the Arkhanes die, the picture is taken one step further, not only pictorially in the creation of space, but also pictographically: several of the other sides give us examples of actual script.

Even in Crete representative art remains a minor theme in seals and pottery through the MM period. Most of the Phaistos sealing motifs are entirely decorative.[47] But on the mainland concepts of equivalence and representation disappear, and concerns with other levels of symbol and other forms take their place. Representative art, after all, is only one kind of response to a need for symbols. It may—rarely—be a close transcription of nature, but even here the learned skills, the nature of the medium, and the place the work was to fill dictated to the artists as much as the flowers or the animals did. The master of the Mochlos and Zakro dog lids (Seager 1912: 21, fig. 5; Platon 1963: pl. 154b; Warren 1969: 82) undoubtedly knew the nature of dogs, but his work is also a superb abstraction of curved forms—rump, tail, and feet, set off against an angular worked ground. Such abstraction is the formal basis of much of the contemporary pottery. To combine the readable and the abstract may be the new contribution of the greatest of the EBA artists, and the theme as well of many others to come.

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[47] *CMS*, II, 5, nos. 1-252; nos. 253-326 are representational.

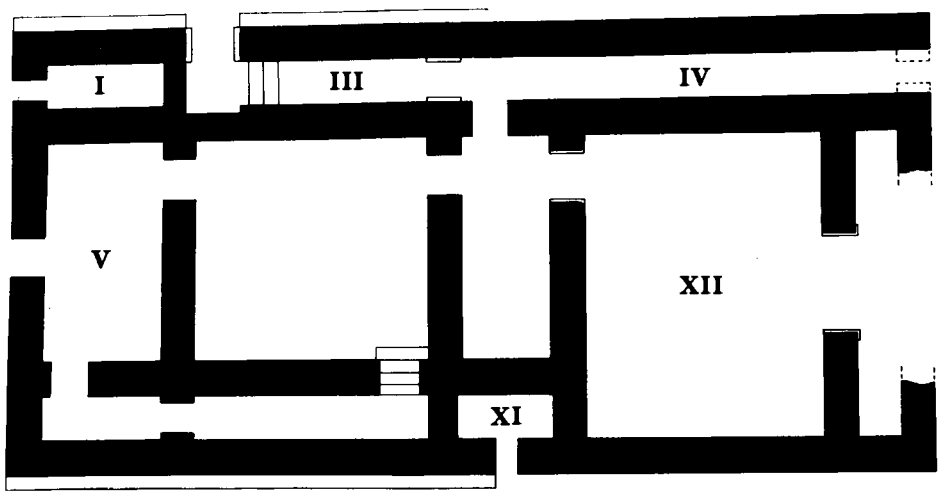
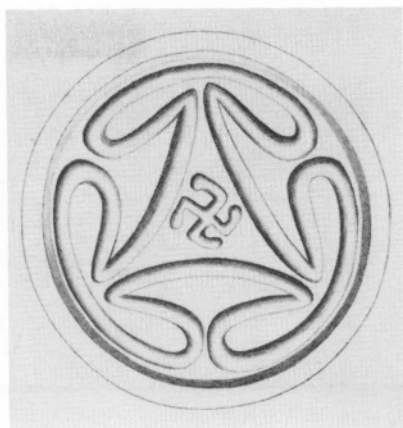
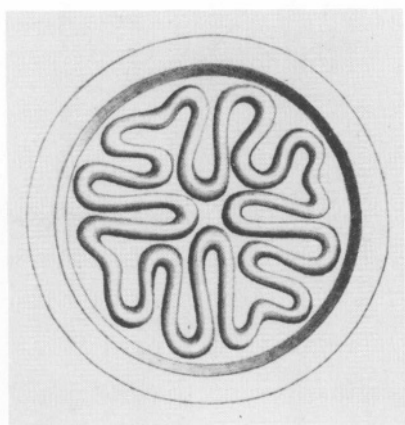


Plate 11

Lerna: House of the Tiles

a.


b.


c.

d.

e.

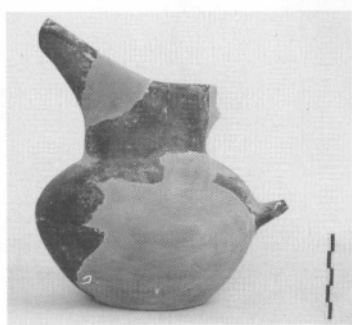

f.

Plate 12

a) Lerna: Sealing, EH II; b) Lerna: Sealing, EH II; c) Lerna: Sealing, EH II; d) Lerna: Sealing, EH II; e) Lerna: Pottery, EH II; f) Lerna: Pottery, EH II.

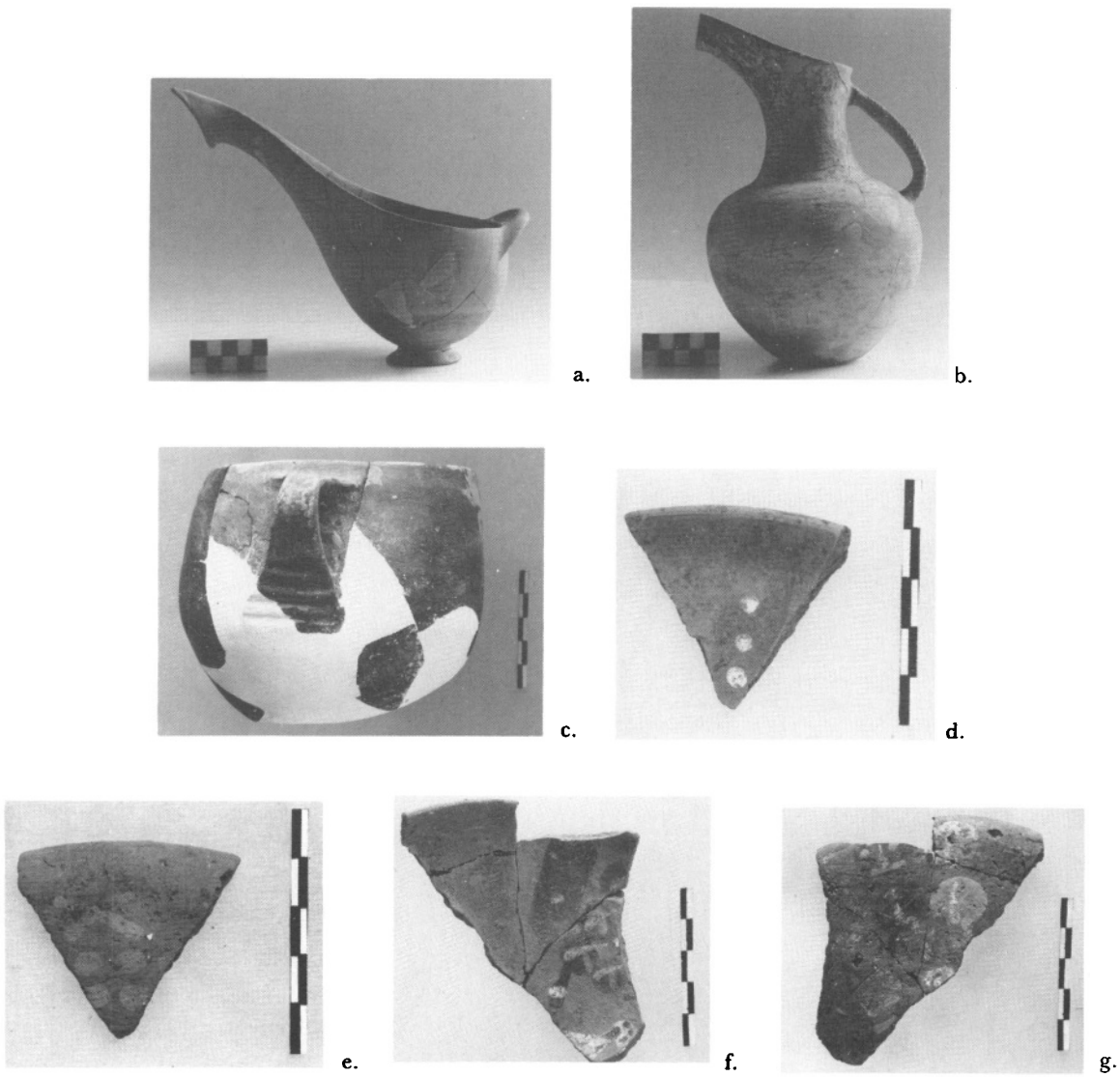
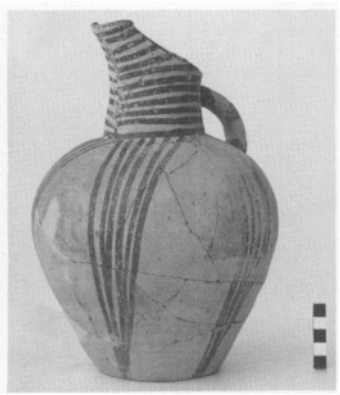


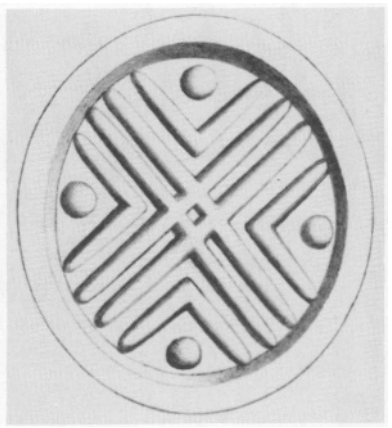
Plate 13

a) Askitario: Sauceboat, EH II; b) Askitario: Jug, EH II; c) Lerna: Sauceboat sherd, EH II; d) Lerna: Sauceboat sherd, EH II; e) Lerna: Sauceboat sherd, EH II; f, g) Lerna: Pottery, EH II.

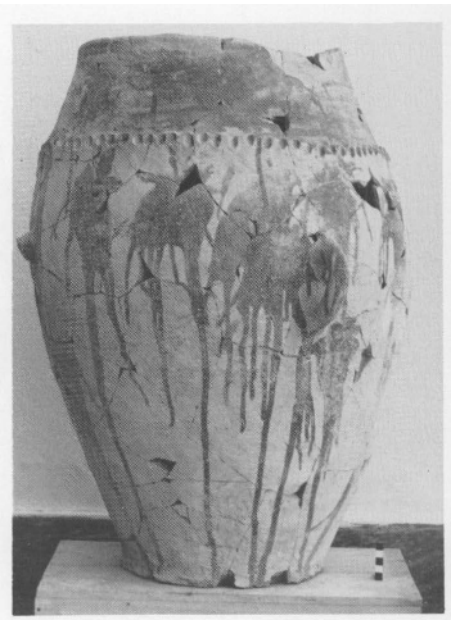
Plate 14
Fournou Korifi



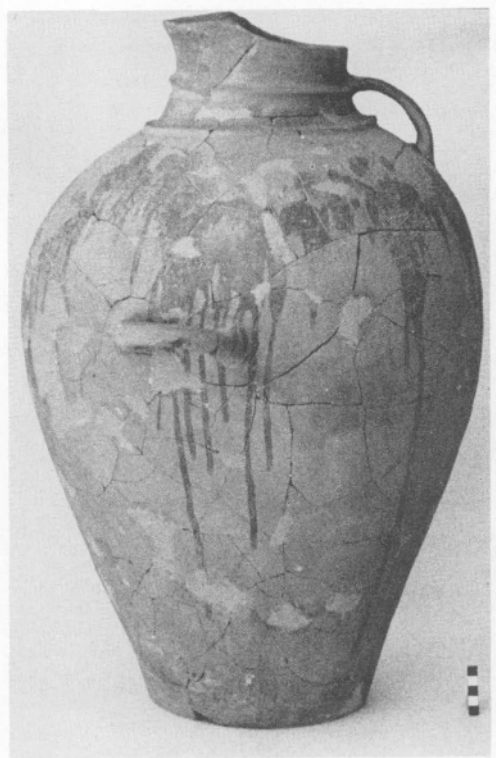
a.



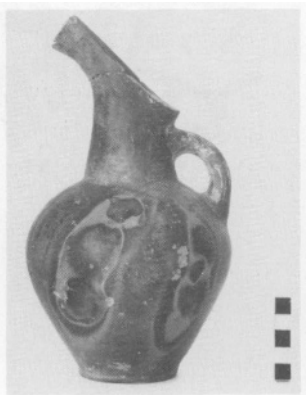
b.



c.



d.



e.

f.

Plate 15: a) Fournou Korifi: Myrtos ware jug, EM II; b) Fournou Korifi: Sealing, EM II c) Fournou Korifi: Pithos, EM II; d) Fournou Korifi: Hydria, EM II; e) Fournou Korifi: Vasiliki ware jug, EM II; f) Fournou Korifi: Myrtos goddess, EM II.

THE LINGUISTIC EVIDENCE: IS THERE ANY?

ANNA MORPURGO DAVIES

1. For the place and period we are interested in, the Early Bronze Age in the Aegean, there is no direct linguistic evidence, in the sense that we have no contemporary written documents from any part of Greece, or indeed any part of the West Coast of Asia Minor. The only conceivable exception could be provided by some texts in Cretan Hieroglyphic and by the few fragments of Linear A or Proto-Linear A found at Phaistos, if the very early date suggested by Levi is accepted—and even so there is no way of going back earlier than MM I. However, a) the dating is always disputed (especially in the case of Linear A at Phaistos), and b) the texts are too fragmentary to be of any use. So much of course will come as no surprise to any one. Unfortunately, most of the content of this paper will also cause no surprise, since it merely provides a restatement of the obvious or, when the problems are controversial, a statement of my own opinion. I have taken as my brief to deal exclusively with linguistic evidence and to make no attempt to rely on data of an archaeological or mythological or historical nature. This is, of course, not a procedure which is likely to lead to any satisfactory conclusion. A conclusions, if any, will come from combining different types of evidence, but in the present semi-chaotic stage of the discussion, when hypotheses and counter-hypotheses are thrown in from all directions without much hope of a cogent demonstration, it may be wise to try to isolate the conclusions which each approach can reach on its own, before we try to adopt a more sophisticated procedure. This will, at least, prevent those perpetual misunderstandings which induce archaeologists and historians to believe that the linguist has the answer—and vice-versa. Even so, the linguist is often reduced to complete silence in this field, unless he can count on some extra-linguistic data. I am no exception to the rule, but when this occurs I shall try to make it explicit.

2. There is no direct linguistic evidence for the Early Bronze Age, but this does not imply that there is no linguistic inference that can be made. What we do have, of course, is linguistic evidence from a later period and we are well accustomed to reconstructing earlier linguistic situations from later evidence.

The first question then concerns the later evidence, its nature, its date, and its geographic distribution.

There are at least three different types of evidence: 1) written evidence which we can read and understand (such as the Greek texts of the first millennium BC); 2) written evidence which we can read (in the sense that we can attribute a phonetic value to the signs of the script) but we do not understand (this is the case, e.g., of the so-called Eteocretan texts of the first millennium); 3) written evidence which we cannot read and cannot understand (an example is provided by the Cretan Hieroglyphic texts of the second millennium). In listing these data I shall limit myself to the classical and pre-classical periods and to what can be loosely called Greek territory. I shall not try, at this stage, to discuss the evidence from Asia Minor, simply because it is too complicated to allow a brief survey; later on I shall have to consider Near Eastern evidence.

2.1. My first type of evidence mainly includes Greek, though this language is attested at different phases of its development and in different scripts. Starting from the eighth century BC we have evidence for alphabetic Greek on the mainland and in the islands. From the same period we have evidence for Greek written in a syllabic script in Cyprus. Very recent findings, though fragmentary, show that the Cyprian syllabary was used as early as the eleventh century BC (Karageorghis 1980: 134-36). At a date, which is not entirely clear, but which is normally put between the fourteenth and the twelfth centuries BC, we have evidence for Greek written in Linear B in Crete and in the mainland.

In the first millennium we also have evidence in Cyprus for Phoenician written in the Phoenician alphabet, but this fact adds little to our knowledge and we shall not mention it again. Similarly we do not need to consider isolated imports of Oriental origin, such as the second millennium cuneiform and hieroglyphic cylinders from Thebes (cf. Touloupa 1964; 1965: 230-31; Falkenstein 1964; Lambert 1964; and now Porada 1982).

2.2. The second type includes peripherical evidence. Nothing comes from the mainland,[1] since I shall ignore the Carian "bilingual" from Athens and the odd miscellaneous objects of oriental origin: in all cases we deal with isolated imports which bear witness to commercial relations, without even giving us any idea of their magnitude.[2]

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[1] The supposed 'Pelasgian' words in an inscription from Pharsalos in Thessaly, first published by Giannopoulos (1919: 50-51), are read as Greek by Peek (1938: 20-27).

[2] For the Carian bilingual see O. Masson 1973: 198 (with the earlier literature). For the reasons given above there is no need to discuss the possible presence of Hieroglyphic Luwian seals in Greek territory: see Jakovidis 1964; 1966; Meriggi 1965; 1966; Boardman 1966; Erlenmeyer 1966; cf. also Güterbock *ap.* Porada 1982: 71-72.

In *Lemnos* we have a large inscription dating from the sixth century and written in a script derived from the Greek alphabet but similar to Phrygian; the text can be easily read but its content remains mysterious. The same script is used in four graffiti on vases, also dating from the sixth century, according to the editor, which are too fragmentary to be of much use. No doubt the language is not Greek; similarity with Etruscan has been repeatedly suggested and, in spite of the sneering remark that all that Lemnian and Etruscan have in common is the fact of being incomprehensible, the chances are that the similarity is real.[3]

Crete is slightly more prolific. As early as the seventh century BC we have from Dreros in central Crete a five line inscription of which the first two lines, though written in Greek characters, are not in the Greek language. The last three lines are in Greek but are very fragmentary. From Praisos, in East Crete, we have six texts (some of which are very fragmentary) also written in the Greek alphabet but not in Greek. The earliest seem to date from the sixth century BC (nos. 1 and 4), the others from the fourth or third centuries (nos. 2, 3, 5), except for a small fragment (no. 6) which is earlier. The language is conventionally called Eteocretan and the same label has been applied, not very reliably in my opinion, to a short inscription (from Psykhro on the Lasithi plain?), dated by Marinatos (1958) to the fourth or third centuries. The text has three lines in the Greek alphabet, followed by some three signs in a fourth line, which are not Greek letters and have been compared, somewhat rashly, to Linear A signs.[4]

Cyprus in the first millennium used its syllabic script both for Greek and for another language, which we conventionally call Eteocyprian. There is some uncertainty in the identification of the Eteocyprian texts, due to the obscurity of the syllabic script. However there is a small nucleus of such texts from around Amathus, Kition and Golgoi. Most of the texts can be dated in the fourth and third centuries

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[3] The Lemnos stele was first published by Cousin and Durrbach (1886). For a better edition see Nachmanson 1908 (= IG XII, 8, 1); more recent bibliography in Rix 1968. For the graffiti on vases see Della Seta 1937.

[4] For the Dreros 'bilingual' see van Effenterre 1946 (1); Lejeune 1947; Jeffery 1949. For a Greek interpretation cf. Georgiev 1947; for a Semitic interpretation Gordon 1966: 8-9; for an Anatolian interpretation S. Davis 1967: 267. Gordon finds a second 'bilingual' in an archaic inscription from Dreros first published by van Effenterre (1946 (i)) and republished by McDonald (1956), but the supposed Eteocretan sequence is far too short and far too doubtful to be much of use (cf. McDonald 1956: 71, n. 10). The Praisos texts appear in IC III, vi, 1-6 (see the photographs in S. Davis 1967: 257); Semitic interpretation in Gordon 1966: 10-11; Anatolian interpretation in S. Davis 1967: 257. The 'Psykhro' inscription was first published by Marinatos (1958); for a new reading cf. Brown 1978; see also Gordon 1966: 13-14; S. Davis 1967: 254 (with a good photograph); Stieglitz 1976. These texts, and a few others, are now collected and discussed in detail in Duhoux 1982, which supersedes all previous editions.

BC and so far they remain incomprehensible, though a few inflectional (?) elements have been identified.[5]

2.3. My third category includes documents which we cannot read and cannot understand. Here too, the data come from peripheral areas: Crete, Cyprus and some of the islands. There is no clear evidence from the mainland; a few odd signs of Linear A appearance, mason marks, etc. have been found but it is difficult to know if they must be treated as writing, and in some instances they may belong to imported objects.[6]

In *Crete*, in addition to Linear B, there is evidence for at least three different scripts, all dating from the second millennium BC.[7]

I shall just mention the Phaistos disc: the origin of this curious object, which is normally dated to a period between MM II and MM IIIB and was found together with a Linear A tablet, is still obscure and there is no agreement whether it was imported or local. It is unique, and—in spite of its too numerous decipherments—is unlikely to provide us with much help.[8] A better localised script is the so-called Hieroglyphic Cretan. Olivier (1976: 17) has correctly pointed out that we know practically nothing about this script except that it is not hieroglyphic and is not pictographic. It is attested—in a scanty manner—in various parts of Crete in the period which goes from MM I to MM III or perhaps even LM I, and, at Mallia and Phaistos at least, seems to overlap, in a somewhat evolved form, with Linear A texts. The few remnants that we have do not certainly allow a decipherment, nor do they allow us to define the connection with Linear A or Linear B. The chances are that there is indeed a graphic connection between Cretan Hieroglyphic and Linear A, but this (a) is not proved, and (b) even if it were proved, would not tell us anything

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[5] For a summary of what is known about Eteocyprian, cf. O. Masson 1983: 85-87, 201-12, 413; Masson 1979: 407. New texts or reeditions of old texts in O. Masson 1966: 27-29; 1971 (cf. Neumann 1973); a list of texts in O. Masson 1979: 369. A vase fragment from Kition, which may contain an Eteocyprian word or name, is dated to the eighth or seventh century BC.

[6] Grumach (1962: 85) drew attention to the discovery of two Linear A signs on a copper vessel from Shaft Grave IV at Mycenae (cf. Hooker 1979: 46). The object may be imported from Crete so that, even if the signs may be treated as a form of writing, this does not contradict the statement made above.

[7] Hooker (1979: 16) recognises "no fewer than seven Minoan writing systems" and mentions (in addition to Pictographic, Linear A, Linear B, and the Phaistos disc) the Arkalokhori axe, a clay tablet from Tylissos (TY 2), and a fragmentary clay tablet from Knossos, all of which show signs which cannot be identified with those of Pictographic or Linear A. The evidence is very small and cannot be assessed with any confidence, so that I feel entitled to ignore the last three objects mentioned.

[8] We should remember that the date of the disc is far from certain. An up-to-date introduction to the problems of the Phaistos disc is provided by Duhoux (1977, with the earlier literature). For a photographic edition cf. Olivier 1975 (1); 1973. A case for the Cretan origin of the disc is made by Hooker (1979: 16-17). Cf. also Heubeck 1979: 6.

about the language thus written. If the two scripts overlapped geographically and chronologically, it is quite possible that they were used for different languages, but even this is not certain.[9]

Finally we have in Crete a great deal of evidence for Linear A. It is not often realised how greatly our evidence has increased in the last few years. Brice's collection (1961) is being replaced by a five-volume edition by Godart and Olivier (1976-) which includes a much larger number of documents. Linear A texts span a period which more or less coincides with MM and LM I; it is obviously the normal form of writing for the second palaces. There is a certain amount of evidence which may indicate that the Linear A inscriptions, which now come from all parts of the island (Zakro in the South East, Ayia Triadha in the centre and Khania in the North West, to give examples) are written in the same language. Certainly the word which means 'total' is used both at Zakro and at Ayia Triadha.[10]

The next port of call is *Cyprus*. Here we have evidence for writing in the second millennium and there is a graphic connection between the writing of the second millennium and the syllabic script(s) of the first millennium. In Cyprus, E. Masson has found evidence for two related forms of script, which she assumes corresponded to two different languages. The two scripts are closely related to each other and (less closely) related to the Cretan scripts.[11] The first script (Cypro-Minoan 1, in her terminology) is attested between the sixteenth and eleventh centuries in various parts of Cyprus and on a number of different objects; the second script is attested in Enkomi only in the thirteenth-twelfth centuries. Finally, a third variety of Cypro-Minoan was in use at Ras Shamra-Ugarit. According to Masson, the second script, which is attested in large tablets, may represent the adaptation of the standard Cypro-Minoan script to the language of a group of newly-arrived peoples, whom she tentatively identifies with the Hurrians.[12]

In the *Islands* we have in the second millennium a few very fragmentary bits of evidence for Linear A at Thera (Marinatos 1971: 64), Keos (Caskey 1970), Melos

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[9] For a recent introduction to Cretan Hieroglyphic see Olivier 1976, and now Heubeck 1979: 2. The richest edition of texts is still Evans 1909, but for the new findings of Mallia cf. Poursat *et al.* 1978.

[10] Pope and Raison (1978) give a very detailed and up-to-date account of Linear A and its problems (with ample bibliography). For a list of editions see Raison and Pope 1977; cf. also Raison and Pope 1980. GORILA, the new edition by Godart and Olivier, will eventually appear in five volumes but is not yet complete (Godart and Olivier 1976-). Also important as introductions are Olivier 1975 (2) and Godart 1976. Cf. also Heubeck 1979: 11.

[11] The earliest Enkomi clay tablet dated at the end of the sixteenth century shows clear graphic similarities with Linear A (Masson 1969).

[12] For a general introduction to Cypro-Minoan see Masson 1974; for an edition of the new texts Masson 1978, for the 'Hurrian decipherment' Masson 1973; 1974; 1977; for Cypro-Minoan and Eteocyprian, Masson 1979. See also Heubeck 1979: 54.

(Renfrew and Brice 1977), Kythera (Coldstream and Huxley 1972: 205-206, pl. 60, fig. 59), and possibly Naxos (Kontoleon 1965) [cf. now Godart and Olivier 1976-, vols. 1, 2, 4].

Finally a few words should be said about Linear A. I have listed the Linear A texts in my type 3, but the position is more complicated. It seems that, if not all, at least a few signs of this script can be attributed a phonetic value with some confidence. In other words, it is reasonable to assume that we can attribute the Linear B values to the similar signs in Linear A, if these occur in sign-groups which also occur in Linear B and which are of such length that the identity is unlikely to be accidental. From an examination of such groups Godart (1976) concludes that it is legitimate to attribute a phonetic value to some fourteen signs of Linear A. Pope and Raison (1978: 40-41) also attribute phonetic values to at least thirteen signs, though not necessarily the same ones as Godart; they conclude that of the 538 words they have considered there are no more than a hundred that cannot be entirely "read". If this is acceptable, Linear A falls exactly in between my second and third type.

The meaning of a few words can be guessed from the context; we know the word for 'total', possibly the word for 'deficit', the names of some vessels, etc. Yet, we still do not know how to interpret the texts. Attempts have been made to recognise in the language of Linear A a form of Greek (e.g. Peruzzi 1960; Nagy 1963; 1965; Georgiev 1963) or of Semitic (e.g. Gordon 1966, etc.) or of Indo-European of Anatolia (e.g. Georgiev 1963; Davis 1967; Palmer 1958, etc.). The value of these "decipherments" varies; at the moment none of them seems to be based on sufficient evidence to carry full conviction and the new texts are obviously changing the original situation. A detailed discussion would lead us too far afield, but I shall have to come back to the "Luwian" hypothesis later on (4.4.4.).

2.4. So much for a basic survey of the relevant evidence in Greek territory. In the first millennium Greek is clearly the dominant language and Greek is also attested in a number of places on the mainland and in Crete in the second part of the second millennium. In the first millennium we also have pockets of non-Greek languages in Crete (Eteocretan), Cyprus (Eteocyprian) and Lemnos. So far nothing points to a linguistic similarity of all these non-Greek languages and it is safer to treat them separately. We ought also to remember that it is not demonstrated that all Eteocretan documents are written in the same language.

In the second millennium we have evidence in Crete for at least one language which in my opinion is not likely to be Greek, the language of Linear A. It is not possible to prove or disprove that the Phaistos disc, the Cretan Hieroglyphic texts and the Linear A texts are written in the same language or in dialects which belong

to different phases of the same language. Similarly we do not know if Eteocretan is at all related to any of these languages.

Also in the second millennium Cyprus may provide evidence for two different languages, if we follow Masson; what these languages were it is not possible to say with confidence. In the other islands of the Aegean Linear A was occasionally used and it is possible that it was used for the same language as in Crete. Given the nature of our evidence no linguistic proof is available nor do we know that the texts were produced locally.

3. What conclusions do we draw from this summary of the evidence? First, we know that Greek is Indo-European, while we are, at the moment at least, entirely ignorant of the linguistic affiliation of the other language(s)—obviously so for those written in a script which we cannot read, unfortunately so for those written in a script which we can read. Secondly—and here I move away from a strictly linguistic approach—it seems likely that Indo-European, or rather that form of Indo-European which became Greek, did not arise in Greece itself, nor reached Greece strictly through diffusion without any movement of peoples. In other words, most linguists still wish to assume that the Indo-European languages were brought into their Mediterranean places at a relatively late stage of their history and that the *Urheimat* of the Indo-Europeans was not in the Mediterranean area.[13] Objections have been raised against the expression "the coming of the Greeks", since it makes little sense to refer to Greek speakers before the period when the language acquired its definitive features, and this no doubt happened in Greece itself. While accepting the point, it is possible to argue that it makes sense to speak of the arrival of Indo-European-speaking groups, which in the course of time altered their form of speech and in Greece started speaking something which we would recognise as Greek.

Given all this, and given the areas of allophony in Greek territory in the first millennium, we may now ask whether Eteocretan and Eteocyprian (I shall leave aside the question of Lemnos) are to be treated as due to the superimposition of foreign speakers on Greek or vice-versa. In other words, is the situation similar to that which led entire sections of New York or Philadelphia to be italophone, or is it comparable to that which confronts a tourist in the United States, who is lucky enough to enter an Indian reservation and to find someone who speaks an Indian language? This is largely a matter for archaeologists and historians to decide; I

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[13] Different views have occasionally been advanced; cf. e.g. Renfrew 1964: 135-41 and 1973. I doubt that any linguist could accept the "possibility that the Greek language developed, of its own accord and without the incursion of destructive invaders, from the pre-Greek substratum" (Renfrew 1964: 141).

should instinctively prefer to assume that Eteocretan and Eteocyprian represent pockets of a language which retreated in front of Greek rather than vice-versa. If this is so, they may continue languages spoken in the islands in the second millennium. However, as I mentioned above, it does not follow that they continue the languages for which we have evidence written in Linear A or Cretan Hieroglyphic or Cypro-Minoan 1 or Cypro-Minoan 2—though this is possible. In addition to this, the linguist cannot have anything to say about the time when the speakers of these languages reached their historical places. Something could be said, if we understood the texts and recognised the genetic affiliations of the language—but this is just what we cannot do (for Luwian see below 4.4.4.).

4. I now turn back to Greek and to the inferences which we can make from the features of this language as we have it in the second and first millennia BC. There are a number of ways in which the problem can be approached.

4.1. The language is Indo-European and, as stated above, this implies, in my opinion, that there was a group of Indo-European speakers which at some stage reached Greece from the *Urheimat*—though obviously nothing proves or even suggests that migratory hoards moved in a straight line from the *Urheimat* to Greece. If we could identify the Indo-Europeans with a particular culture which has left archaeological remains of one or the other type and is, for instance, associated with a particular type of pottery or a particular type of burial, it would be possible to say something about the arrival of the Indo-Europeans in Greece and its chronology. Yet in this linguistics does not help. Hypotheses have been made about the archaeological aspect of the Indo-Europeans, but there is no agreement about them and I find it difficult to see how, in the absence of any written evidence contemporary with the relevant period, a purely linguistic concept can be confidently associated with archaeological data: in other words I share some of the misgivings expressed by Schmitt (1974). The problem concerns more the archaeologist than the linguist and, in view of the disclaimer made above (1.), I cannot tackle it here; any solution is fraught with difficulties and by its very nature is such that the linguist cannot use it as the foundation of *his* edifice.[14]

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[14] A useful collection of papers on the subject was edited by Scherer (1968); for a historiographical introduction to the Indo-European problem see Mallory 1973; for a bibliography and a recent reappraisal of the situation, Crossland 1971 (1), 1972 and 1973. Schmitt's sceptical remarks called for a reply by Gimbutas (1974); cf. also Gimbutas 1970 and, in general, most of the essays collected in Cardona *et al.* 1970. Wyatt (1970) has argued that the linguistic evidence shows that the Indo-Europeans arrived in Greece ca. 1600. I suspect that his data could be explained in a different way.

4.2. Another approach also starts from Greek but in a more modest way. If we accept Ventris's decipherment of Linear B it follows that there were Greeks who wrote Linear B in Greece in the last part of the second millennium BC. It is well known that in the first millennium Greek is fractioned in dialects and that in some instances their geographic distribution can only be explained through a number of historical assumptions. The similarities between Ionic and Attic, on the one hand, and between Lesbian, Boeotian and Thessalian, on the other hand, can be accounted for if we assume that groups of people moved from the mainland to the coast of Asia Minor sometime during the period of the Greek colonial expansion.[15] This is a clear-cut instance, but there are more complex problems. We wonder why Arcadian is so closely related to Cyprian, though it is so isolated in the centre of Peloponnese, surrounded as it is by Doric and West Greek speakers. We can guess that the ancestors of the Arcadians occupied large parts of the South and West Peloponnese and perhaps extended their sphere of influence as far as Cyprus. This hypothesis can be made on purely linguistic grounds; the close connection between the Mycenaean language, as attested in the Linear B texts, and Arcado-Cyprian now confirms this view. The example is relevant because it shows that an analysis of dialects and their distribution may lead to sound historical conclusions. Is it possible to explain in a similar manner the existence and distribution of the main dialect groups of Greek: Aeolic, Ionic-Attic, Doric? According to the traditional explanation, which is based on such an assumption, the tribes which brought Indo-European to Greece arrived in waves separated in each case by a number of years. The three main dialect groups represent each a different wave of invaders. If that were so, and if we could assume that the authors of our Mycenaean texts were the last-but-one wave of invading tribes (preceding the Dorians only), it should be possible to date the first wave to a relatively early period, possibly as early as the end of EH or the beginning of MH. Presumably a linguist could produce this date only as a *terminus ante quem* and not as a *terminus post quem*, since the whole process, including the arrival of the future Mycenaean speakers, could have taken place at any time, albeit in the staggered manner described above. The conclusion would be that some speakers of the Greek-to-be language, if not all, ought to have been there at the beginning of MH. Unfortunately this hypothesis rests on what I called the traditional view of the distribution of the Greek dialects. In recent years it has become clear that, in spite of the parallels mentioned, which associate the presence of given

[15] On purely linguistic grounds we could probably postulate an original unity of Attic and Ionic, but I doubt that we would be able to establish the direction of the migration which caused the separation.

dialects in certain areas with extensive migrations or colonizations, nothing obliges us to assume that the same happened in the case of the pre-Doric dialects. In other words, it is conceivable that the Indo-European speakers arrived in Greece as a group or in scattered tribes which moved very closely to each other, and that the dialect distinctions arose in Greece itself at any period before that of our first evidence.[16] If this is so, the dialect distinctions of Greece do not allow us to make any historical inference about the first part of the second millennium B.C.

4.3. The first two approaches, as we have seen, are not very fruitful. We must now examine a rather different way of considering the problem, which is based on a closer analysis of Greek as we know it. Let us remember first that part of our purpose in examining the linguistic evidence provided by non-Greek documents of the first and second millennia was to see whether we could establish what language or languages existed in Greece at an earlier time and to try to reconstruct through a study of its or their affiliations some part of the history of the original speakers. The written evidence we have proved unsatisfactory, but it could be argued that better results could be obtained through a close examination of Greek itself and of the influence of other languages on it.

Suppose, to invent a paradoxical example, that Greek was shown to have been influenced by Finnish (or rather by an ancestor of Finnish) as closely as English has been influenced by Latin; this would automatically call for some explanation. Since the influence is unlikely to have occurred in the historical period (we would know the circumstances) it ought to be prehistoric. In a period in which writing is not a normal part of life, linguistic influence, if it exists at all, implies close contacts and perhaps a certain amount of bilingualism. Where did the contacts occur, how could we bring together the predecessors of the Greeks and the predecessors of the Finns? The example is paradoxical (to my knowledge Greek shows no trace of Finnish influence), but it may make clear what sort of influences and contacts we are looking for, and it may also show how, if these were identified, they could lead to some sort of historical conclusion.

4.3.1. The first question concerns the way in which we ought to proceed. In obvious cases, such as that of the Latin influence on English, we know Latin and this simplifies the problem. Assuming we did not know Latin, would we still recognise the words which English borrowed from Latin as in some way "non-English"? The

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[16] The traditional view is expressed in a number of articles by Kretschmer (e.g. 1909). For modern introductions to the problem (with the earlier literature) see Chadwick 1975; Schmitt 1977: 124-33. Cf. also Palmer 1980: 57-82.

question becomes more general and concerns the way in which we identify loan-words, or at any rate foreign elements in a language. Morphological elements are both easier and more difficult to recognise but are less common. There are a number of criteria which can be used to recognise loan words: I shall list a few here.

First, loan words or forms may patently break the phonological or morphological rules of the language. This is the case of some pronunciations of a word like *garage* in British English [gæ'ra:ʒ],[17] or of some names like Δαυίδ which appear in late Greek under Biblical influence.

Secondly, loan words or forms do not have an obvious etymology in terms of the parent language, even in languages which are transparent enough to allow an etymology of most lexical items. In English, a word like "zero" cannot be explained in terms of Proto-Germanic.

Under this heading we may distinguish a number of different cases.

(a) The language of origin may be known and the word in question attested in it: "zero" was taken from French into English and from Arabic into French.

(b) The language of origin may not be identifiable, but the word belongs to a group of words or forms which (1) cannot be etymologized, but (2) share some semantic or grammatical features. Even if we did not know Italian, we could see, for instance, that a large part of the English musical terminology is both non-etymologizable in Old English terms and shares some phonological and morphological features such as the frequent vocalic endings (*piano*, *lento*, etc.), the *-issimo* suffix (*pianissimo*, *lentissimo*), etc. A combination of these features with the fact that all the words in question are concerned with music makes it likely that we are dealing with an instance of foreign influence on English and that all words come from the same language.

It is also clear that the absence of an entirely convincing etymology is not in itself sufficient to guarantee that a word is of foreign origin (the problem may be different for morphological elements); it simply spurs us to look round and see if there is any other reason (such as those mentioned under (a) and (b) above) to assume that the word is foreign.

Thirdly, we may find that some words or forms (which we shall want to call loan words or forms) can intuitively be compared with words or forms of the parent language (i.e. can intuitively be etymologized in terms of words or forms attributed to, or reconstructed for, the parent language), but do not show the normal phonetic development. The English *dentist* can be intuitively compared with the Indo-

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[17] Notice, however, that we are able to observe this fact only if we have at our disposal spoken rather than written English—a condition which can hardly be satisfied for ancient Greek.

European word for 'tooth', **dent-*, but we expect an Indo-European **d* to be continued as *t* in English, and an Indo-European **t* to be continued as *th* (as in *tooth* < **dent-*). We happen to know that *dentist* is of French and, ultimately, Latin origin. If we did not know it, we would be interested in collecting other words (*decimal* vs. *ten*, *duodecimal* vs. *two*, *paternal* vs. *father*, *fraternal* vs. *brother*, etc.) where the same phenomenon occurs: on the one hand there is a similarity, intuitively established, between the English words and the Indo-European equivalents (**dekm̥* '10', **dwṓ* '2', **pH₂ter* 'father', **bhrāter* 'brother', etc.); on the other hand there are numerous 'irregular' sound treatments, which may even lead us to establish a new set of 'regular' rules. In other words, though we decided earlier that Indo-European **d* and **t* were continued by English *t* and *th* respectively, we now find a series of words where in similar environments they are continued by English *d* and *t*. It must be stressed that the observation is important only if it applies to a series of words. If our first example (*dentist*) were unique, it could not take us very far, because we should never know whether the similarity in meaning and form of *dentist* and IE **dent-* is accidental or not. On the other hand, if a series of words is available, we must seriously ask whether English shows the influence of another Indo-European language. The nature of the words in question can also tell us something about the character of this influence. If the words belong to a literate society and are predominantly learned words, we may think of literary and cultural influence and we do not need to postulate contacts of peoples (at this stage at least)—though obviously a certain amount of bilingualism is the necessary precondition for any instance of large scale borrowing. In the case of words which indicate plants, animals, etc. or are, at any rate, tied to a particular geographic or climatic condition, we may think of large scale borrowing from a substratum language, the language originally spoken in the area.

Finally, a few words must be said about personal and geographic names, since they occupy a very special position. It is commonplace to notice that place-names in particular often remain tied to given places, even though the language of the local inhabitants may change. The United States has a number of towns with Indian names, though no American Indian language is spoken there. Similarly, I doubt that much Celtic is spoken in Dover in England, though the town has a Celtic name. The general tendency is to assume that place-names preserve traces of the language of the first inhabitants of a given place. Indeed, the Celtic place-names scattered through England document a Celtic occupation of the country for which we have other evidence too. Yet, a warning is necessary—and has often been given—against the danger of forgetting that place-names can also be carried round or acquired by people who do not speak the language to which the name belongs. The existence of

Philadelphia in the United States does not document a large colonization of the country by Greeks and the *Cam-* element present in the name of Cambridge, Mass. does not tell us anything about the presence of Celts in Massachusetts.[18] More seriously, we seem to have evidence in Brazil for the spreading of place-names which originally belonged to the local Tupi language through the activities of a number of Portuguese speakers who, in their hunt for gold, gave Tupi names to forests, mountains, valleys and rivers, so that we have Tupi names in regions where there never were any Tupi speakers.[19]

4.3.2. This was a long digression, but we now come back to Greek. We can look for possible "non-Greek" words in Greek following the criteria indicated above. In early Greek we have no clear-cut evidence for words of the first category. Most words seem to be well integrated in the phonological and morphological system of the language—with the possible exception of Linear B names and lexical items, for which, however, the spelling is such that we cannot venture any hypothesis about the pronunciation.

The second and third categories are, of course, relevant. A cursory examination of 578 words listed as lemmata in ten sequences of ten pages each taken from Chantraine's *Dictionnaire Étymologique de la Langue Grecque* (1968) shows that ca. 52.2% of the words have an "*étymologie obscure*". Of the remaining 47.8% almost one sixth (i.e. *c.* 8% of the total) consists of clear or relatively clear borrowings for which the language of origin can often be specified. This leaves us with less than 40% of words which have an Indo-European etymology. Admittedly, the figures are biased because of the particular arrangement of Chantraine's dictionary where e.g. all derivatives and cognates of μιμνήσκω are listed under a single heading so that in my reckoning they count as one word only, just as the obscure μογγάς, which is a gloss with no known derivatives or cognates within Greek. Even so the figures are remarkable.[20]

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[18] Cf. e.g. Chadwick 1969: 84-93. Patterns of preservation and replacement may vary from place to place and from period to period. Hainsworth (1972) points out that in England big cities and rivers tend to preserve Celtic names while minor villages do not: on the other hand Findikoglu (1961) argues that in Turkey the few place-names of Turkish origin belong to large urban centres only—though in a sense the Turks are the newcomers on the Anatolian scene. Finally Robert and Robert (1977: 18) observe *à propos* of Anatolia that "*La persistance de la toponymie antique est un fait très important ... Il s'applique ... ainsi bien à la ville importante et sans éclipse ... qu' au dernier des villages*".

[19] Cf. e.g. Chaves de Melo 1975: 41.

[20] My calculations are based on the words listed in Chantraine 1968: 100-110 (ἅπτω - ἅρμα), 200-210 (βρύω - γάγρα), 300-311 (p. 307 is blank) (δρύς - ἑδανός), 400-410 (ζέω - ἥλιος), 500-510 (καρπάλιμος - κεκαδών), 600-612 (pp. 607-8 are blank) (κυνέω - λαδρέω), 700-710 (μηχανή - μολύνω), 800-810 (ὁμός - ὀπυίω), 900-910 (πιέζω - πλάσσω), 1000-1010 (σήσαμον - σκαπέρδα), 1100-1110 (τέκμαρ - τευμάομαι).

We have seen that *c.* 8% of our sample is composed of words treated as possible borrowings from a number of languages. According to my earlier classification most of these words would belong to the (a) section of my second category. Also, most of the words in question were introduced into Greek at a late stage and do not interest us here. Yet, it may be worthwhile to remember that the Mycenaean texts have shown that Greek borrowed a number of Semitic words before our first texts, i.e. that there were close contacts with Semitic peoples long before the Dark Ages. Most of the words borrowed refer to herbs or plants, perfumes, textiles and precious objects, which may have been introduced through trade relations, It is possible that at some stage a few abstract words were also borrowed but, even so, though some bilingualism was obviously called for, it is not easy to argue that this bilingualism was due to the presence of a Semitic substratum or superstratum in Greece.[21]

Anatolian borrowings are also attested in Greek, as recent studies have shown. In most cases the date of introduction of the words is not clear, and here I shall only mention that there may be recognisable Anatolian loan words in the Linear B texts: ἔλεφας 'ivory' and κύανος 'kyanos' are the main examples.[22] Here too, neither these words, nor the supposed later borrowings clarify the nature of the contacts between Greeks and Anatolians. In the first millennium the Greeks came frequently across the Anatolians in Asia Minor, but for the early period all that we have to go by are

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A few words have been ignored even though they occur as lemmata in the pages listed above; they are either proper names or glosses whose existence and meaning are entirely uncertain. Of the remaining 578 words considered, 301 have no known etymology (I have included among these onomatopeic words, *Lallnamen*, etc.), 229 are built on roots which seem to have equivalents in other Indo-European languages, 48 are treated as borrowings from a specified language or language group. Chantraine is extremely cautious in committing himself to an Indo-European etymology; sometimes, albeit rarely, I felt it was right to ignore some of his cautionary remarks.

Within the sample the proportion between my three categories varies; hence the need to choose at random a number of ten-page sequences from the dictionary. The maximum contrast is between the figures of pp. 400-410 (22 'obscure' words, 28 'Indo-European' words, 3 'borrowings') and those of pp. 1000-1010 (56 'obscure' words, 6 'Indo-European' words, 8 'borrowings').

[21] We should not forget, however, that Gordon has argued that the Cretan scripts were used for a Semitic language (e.g. Gordon 1966) and that Astour (1967) has tried to point out a number of connections between Greek and Semites (especially in the mythology and the proper names). A list of acknowledged borrowings from Semitic is available in Masson 1967 (with the earlier literature). More recent bibliography and some new suggestions in Szemerényi 1974: 145-47.

[22] Cf. especially Neumann 1961; Heubeck 1961; for further suggestions see Szemerényi 1974, 152-55. Laroche (1965) pointed out that Hittite *laḫpaš* must have been the source of Greek ἔλεφας, Mycenaean *e-re-pa*; but see also Laroche (1968: 777) who wonders whether ἔλεφας and *laḫpaš* derived independently from the same source (Syrian?). Hittite *kuwannan* means 'copper', but if preceded by the NA_4 determinative indicates a precious stone. The exact referent of Mycenaean *ku-wa-no* is not certain (see Halleux 1969) but it seems very likely that *ku-wa-no* and *kuwannan* have the same origin (cf. Laroche 1966 (2): 180); Houwink ten Kate (1973: 143) is perhaps too cautious.

the few words of Linear B date quoted above (for other possible contacts with Anatolia, see below 4.4.4.).

4.3.3. The words of unknown etymology which cannot be treated as borrowings from any known linguistic group are very numerous. Do they belong to any special semantic or grammatical sub-group? The answer cannot be firm. It is certainly true that a large number of these words refer to concrete objects, fruits, plants, animals, birds, fish, etc. It is also true that some of the words show consonantal and vocalic alternations which are not normally found in words of clear Indo-European origin: cf., for instance, γρῖπος/γρῖφος 'fishing basket', and κόρκορος/κόρχορος 'the name of a plant, a blue pimpernel' (Arist. and Theophr.). On the other hand it has also been said (Furnée 1972: 104-05) that in most cases we have no perfect doublets but only words which are similar (and may be related), but show different suffixes or different vocalism, so that the alternations cannot be taken as a certain indication of an 'abnormal' phonological structure.

Scholars are divided in their approach to the words of unknown etymology. For a long time linguists have argued that those words which had meanings connected with particular climatic and geographic conditions and no known etymology were survivals of a substratum language, which was spoken in Greece before the arrival of the Indo-Europeans. It has also been suggested that some of these words—and in particular those which emerge in an intuitively similar form in Latin (*vinum*, *menta*, *ficus*, *rosa*, *lilium*, etc.)—represent a wider 'Mediterranean' substratum, i.e. a language which was originally spoken both in Greece and in other Mediterranean countries (cf. Hester 1968: 223-24; Katičić 1976: 1, 56 with references).

Here it is not possible to go into the details, but two points may be made. First, it is entirely probable that some of the words of unknown etymology represent survivals of one or more pre-Greek languages, and that the words which indicate typical Mediterranean plants, animals, etc. belonged to a language or to languages which were more 'local' than Indo-European was. Yet this can only be stated on general grounds without a specific demonstration. Secondly, it is also true that if this were so, and if we were able, for instance, to reconstruct some features of the language or languages in question, no historical conclusions could be drawn from this. We would have no way of dating or localizing the language(s). Any such language, if it existed, could have left its traces on Greek, both if it was its immediate predecessor and if it was the predecessor once or twice or thrice removed. Moreover, the language could have been spoken in one part of Greece only or in all of it: nothing would allow us to decide. In other words, both if we assume and if we do not assume the existence of a Mediterranean substratum in Greece, this tells us nothing about

what was going on in the area in the Early Bronze Age. Nor are we really in a position to decide if we are speaking of one or more languages.[23]

4.3.4. Other linguists have isolated a group of words of unknown etymology, and tried to show that the words can in fact be provided with an Indo-European etymology, if we assume that they show a phonetic treatment of the sounds of the parent language which differs from that attested in the normal Greek words. The phenomenon would be parallel to that which I have illustrated for English with the example of *tooth* and *dentist*. The starting point is provided by some Greek words of certain Indo-European etymology, besides which we also have apparent doublets with similar or identical meaning but a different phonetic aspect; cf. ἀλείφω 'anoint' and λίπος 'fat' with a φ / π alternation; ἄμβων 'crest of a hill, rim of a cup' and ὄμφαλος 'navel' with a β / φ and an α / ο alternations, or also τάφος and τύμβος 'tomb'. From a few examples of this type it is possible to extract some general rules of derivation (e.g. IE **bh*>*b* rather than *ph* as in Greek; IE **p*>*ph* and not *p* as in Greek, IE **o*>*a* and not *o*, etc.); having accepted the new rules, we can then hunt for Indo-European etymologies even for words for which no doublet is available. Thus it can be suggested that the Greek βρέτας 'wooden image of a god', which Chantraine (1968; s.v.) labels as "*terme mediterranéen sans étymologie*", can be compared with the Indo-European root **bhredh-* 'to cut', which yields Germanic **bred-* and German *Brett* 'plank, table'. In this way we multiply the number of Indo-European words in Greek, and we argue that the new phonetic rules apply to an Indo-European language which preceded Greek in Greece. The names given to this hypothesized language differ; the most commonly used is "Pelasgian" and from now on I shall use this denomination in a purely conventional manner.[24]

Before entering into the merits of the Pelasgian controversy, we may mention why it is relevant here. The point is often made that if we were able to prove that in addition to Greek we have evidence in Greece for another Indo-European language, this would be likely to be the language spoken in the area before the arrival of the future Greeks. In other words, our chronology ought to allow for the arrival of two groups of Indo-European speakers, who had developed two remarkably different forms of speech. If so, the existence or nonexistence of the Pelasgians would have historical importance. However, it can be argued that even if Pelasgian existed, all that we

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[23] For a general introduction to these problems and their bibliography see Hester 1968. Very good surveys can also be found in Furnée 1972; Sakellariou 1977; Katičić 1976.

[24] For Pelasgian see, in addition to the surveys quoted in n. 23, Hester 1965. Cf. also Birnbaum 1974. I have not mentioned in the text the diverging, though not too dissimilar, theories of e.g. Merlingen, Haas, van Windenkens, but details will be found (together with the relevant bibliography) in the bibliography quoted above. My examples are all taken from Georgiev 1966.

would really know about it would be that it had great influence on Greek. Influence in this context implies contacts and bilingualism. But, and this is the important point, nothing would imply that the contacts happened in Greece. It would be conceivable, for instance, that the Greeks-to-be and the 'Pelasgians' met during the course of their migrations in 'Pelasgian' territory and that this territory was not in Greece. My conclusion is that even if the existence of the Pelasgian language were established on linguistic grounds this would not be in itself sufficient proof of the existence of an Indo-European substratum in Greece. Yet, in my opinion the existence of a Pelasgian language is also doubtful. The doublets quoted above are at first sight impressive, but either their semantic identity is not certain (cf. ἄμβων and ὄμφαλος) or they can be explained in Greek terms, postulating e.g. different Indo-European 'suffixes' or 'enlargements' (cf. ἀλείφω and λίπος), or they are problematic both for the Hellenist and the 'Pelasgianist' (how do we explain τύμος near τύμβος?). The supporting etymologies are also doubtful and sometimes contradictory (from a root **bhredh-* we should expect **predas* and not βρέτας, according to the 'Pelasgian' sound laws). Obviously it is impossible to demonstrate that Pelasgian did not exist, but it is possible to say that its existence is not demonstrated.

4.4 Where do we go from there? A recurrent problem, which I have already mentioned more than once, concerns localisation and dating. Even if we could establish that the Greeks were influenced by a 'Mediterranean' or a 'Pelasgian' language, how could we date the time at which these languages were spoken or identify the areas where they were spoken? I shall now consider some methods which would allow us to do just that for 'Mediterranean' or 'Pelasgian', if they existed, or for any other pre-Greek language.

First, we could identify the language in question with one of the non-Greek languages attested in the Greek territory: Eteocretan, Eteocyprian, the language of Linear A, the language of the Cretan Hieroglyphs, Cypro-Minoan 1 and 2, the language of the Lemnos stele.

Secondly, we could collect the references made by the Greeks to the peoples they thought had existed in Greece before them or together with them, examine their names, and decide to what linguistic group they belonged on the strength of their etymologies—and, of course, of the assumption that the Greeks used the indigenous names for these peoples.

Thirdly, it should be possible to analyse the known place-names of Greek for which a Greek origin seems excluded and to connect them with one of the reconstructed or attested pre-Greek languages.

The second and third method suffer of an obvious disadvantage. They are based on the etymology of ethnics or place-names. Etymology in its turn is based on com-

parison and comparison must be tested on semantic and phonetic grounds: two words are related when the meanings are similar and the sounds correspond according to given rules. In the case of an ethnic like Πελασγοί we do not know what meaning to look for so that the semantic test is not available. It is not necessarily useful to look for a meaning which fits the cultural or anthropological features of a people, since the name could have been given long before those features were developed or could have been inherited from previous populations which did not share those characters. Consequently, I do not feel inclined to pursue my second method, though I am aware that it has been extensively used.[25] Etymologizing place-names is also dangerous, though we may find meanings which correspond to some unchangeable geographic characteristics ('steep', 'rocky', 'salty', etc.). This does not reckon with the movement of place-names which can be transported from one place to another and may be re-used for emotive reasons, even when they are no longer understood.

As for the first method, it can only be applied to languages written in a script which can be read (Cypro-Minoan and Hieroglyphic Cretan are excluded); even when this condition is satisfied, the method cannot be easily exploited when the evidence is minimal or incomprehensible (as for Eteocretan, Eteocyprian, the language of the island of Lemnos etc.).

4.4.1. In spite of these objections, suggestions have been made aimed at drawing firm historical conclusions; in what follows I shall discuss three different views which have been held in the recent past or are being held at the moment.

In all three theories place-names play a considerable part. Obviously, typically Greek names of the Νεάπολις or Ἀλεξάνδρεια type are not very informative; the main area of interest is formed by the 'unetymologizable' place-names, which traditionally have been taken as Pre-Greek: on the one hand, forms like Ἀθῆναι and Θῆβαι, on the other hand forms distinguished by typical suffixes such as -νθος, -σσος or -ττος, -μνος or -μνα, -ρνος, etc. We may repeat here that there is no reason to suppose that all non-Greek names have the same linguistic background; nothing tells us that Ἀθῆναι and Κόρινθος originally belonged to the same language. However, Hester (1957) has argued that all pre-Greek names formed with the suffixes -νθ-, -ρν-, -μν-, -νδ- (possibly -ττ-), and perhaps -σσ- and -ντ-, show a marked preference for open syllables in the root, beyond a level which can be attributed to chance. This may

[25] Sakellariou (1977: 101) derives the names of the Pelasgians from that of a god Pelasgos and makes a case for a 'Pelasgian' etymology of the god's name, which would derive from **bhel-ozgho-* '*rameau ou branche à fleurs*'. I am not convinced that there is sufficient evidence to prove that the name of the Pelasgians derives from that of Pelasgos and Pelasgos has the meaning it is supposed to have.

speak in favour of attributing these names to one and the same language, but does not prove it. There may have been two pre-Greek languages with a preference for open syllables. Evidence has also been adduced to show that these suffixes belong to a coherent derivational system and are added to the same pre-Greek roots (Λάρυνθος, Λάρισα, Λάρυμνα, etc.). Unfortunately the data are too uncertain and altogether too scanty to offer a definitive proof (cf. Schachermeyr 1954: 1511; 1955: 239; Katičić 1976: 1, 50).

4.4.2. The 'Pelasgianists' have no hesitation in recognising some 'Pelasgian' place-names in Greece and do so for two reasons. First, the root of the names can be etymologized and shown to be Indo-European, but characterized by the typically Pelasgian sound changes. Hence the name of the mountain Ida is derived from the Indo-European root **widh-* continued by the O.Ir. *fid* ''wood', O.E. *widu* 'wood', etc. Pergamos is connected with a root **bhergh-* 'mountain', which through the normal Pelasgian sound changes yields first **bergh-* (dissimilation of aspirates) and then *perg-* (consonantal mutation). Secondly, some suffixes can also be shown to be Indo-European, provided the Pelasgian rules of sound change are adopted. Hence -ινθος in Κόρινθος etc., is derived either from an IE *-n-* stem + a dental suffix, or from an IE **-nt-* suffix or from an IE **-went-* suffix. -νθος must be Pelasgian because in Greek these three sources would yield *-nt-* and not *-nth-*. On the other hand the -σσος/-ττος suffix is treated as partly Pelasgian and partly Greek; in all instances it is derived from a combination of IE **-yo-* and the final consonant of stems which ended in *-t-* or *-k-* or *-s-*. A form like Σφηττοί, the name of the Attic deme, is treated as a Greek derivative of σφήξ 'wasp' and derived from **sphēk-yoi*, but a form like Αμνισός is treated as Pelasgian and derived from **a(m)bh-ni-s(y)o-*.[26]

The whole theory rests on two assumptions: a) that 'Pelasgian' existed as a pre-Greek language and 'Pelasgian' sound changes can be recognised, b) that place-names can be safely etymologized. I do not believe in b) and I have argued that a) is not proved. Hence, I find it impossible to accept the theory and to provide the necessary localization for Pelasgian and the Pelasgians.

4.4.3. The second theory has been differently articulated but mainly rests on the assumption that the non-etymologizable place-names of Greek belong to a pre-Greek, non-Indo-European population. The names considered are Ἀθῆναι, Θῆβαι, Κόρινθος, Παρνασσός, etc. Among them some are characterized by the suffix -ινθος, and it was soon recognised that this also occurred in common nouns, most of which are also non-etymologizable in Greek terms. Since most of the -ινθος words indicate

[26] The examples come from Georgiev 1966, but cf. also e.g. Georgiev 1961 and 1973.

plants, fruits, birds and very concrete objects (cf. ἐρέβινθος 'chick-pea', ὄλυνθος 'wild fig', πλίνθος 'brick', etc.), this seemed to speak in favour of a suffix typical of the Aegean area (but cf. Szemerényi 1974: 152). At the same time the observation that a number of the place-names under discussion was attested not only in Greek territory but also in Asia Minor favoured the view that they belonged to a wide substratum which existed not only in parts of Greece, but also in parts of Asia Minor.[27] The attempt to define this substratum in archaeological terms and to anchor it in time was made by Blegen and Haley in two articles published in 1928. Here it is enough to remember that Haley (1928) tried to identify and to put on a map all place-names of pre-Greek character which were attested *both* in Greece and in Asia Minor. He dedicated a first list to those names which occurred in Greece and in Asia Minor in the same form (Θῆβαι, Πέργαμον, Κορησσός, Λάρισα, Λάρυμνα). A second list dealt with names which were not attested in both areas but were formed with suffixes typical of both areas: -νθος, -σσος/-ττος, -μνος/-μνα, -λ-, -μ-, -ν-, -ρ- ('Αμάρυνθος, 'Αμνισός, Γαργηττός, Λίκυμνα, 'Ιτανός, Δρηρός). The third list included names built on roots which appeared in both areas. When the names were put on the map of Greece it emerged that they covered a well defined area, which left blank a large part of western Greece and northern Greece, but included East and central Crete, the Peloponnese (except for Arcadia and Elis, which had only a few examples), and Achaia, Attica, Boeotia, Phocis and Thessaly. Blegen (1928(1)) compared the distribution of the place-names with that of early Greek sites. He excluded the possibility that the onomastic layer went back to the Neolithic period, because the names were attested in the Cyclades, where there did not seem to be any archaeological evidence for the Neolithic period. He also excluded all the Middle Bronze Age because there were wide differences in culture between Crete and the Greek mainland, while the names covered both areas. His conclusion was that the origin of these place-names was to be found in the Early Bronze period—a map of the excavated Early Bronze sites appeared to cover an area similar to that of the map of names and locations. Blegen also assumed (1928 (1): 148) that "if we can identify the pre-Greek layer, it naturally follows that we can recognise as Hellenic the succeeding layer and we can thus determine exactly where in the archaeological stratification the Hellenes first appear". He concluded that the Greeks arrived at the end of the Early Bronze Age.

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[27] Histories of the question can be found in most of the literature quoted in n. 23 and n. 24; cf. especially Katičić 1976: 1, 40 and see also Lopez Eire 1967. For the earlier period the standard book is Kretschmer 1896. More recently see especially Schachermeyr 1954: 1511; 1955: 239; 1967: 12; 1972; for the *-nth-* formations see Quattordio Moreschini 1984.

In spite of the immense prestige enjoyed by this work, there has also been some serious criticism and I shall mention here some of the objections which may be made. First, even if Blegen was correct in his conclusion about pre-Greek names, it still would not follow that the Greeks arrived immediately after the disappearance of the people who created the place-names in question. Nothing would exclude the existence of another intermediary population through which the names could have been transmitted to the Greeks. It might be objected that, if this was the case, we could have expected to find traces of this population in other surviving place-names which belonged to its language—and we do not. Yet, there is no reason why we should—or at least why we should find such names in Haley's list, since this included only names which are attested both in Greece and in Asia Minor. If our supposed population did not spread to Asia Minor, it would not have left traces in the place-names listed by Haley. But there is a more fundamental objection. Haley and Blegen argued that the toponomastic agreements between Greece and Asia Minor were too many to be due to chance. This is true, but let us remember first of all that the Greeks were in contact with Asia Minor during all the historical period. Now, there may be reasons for arguing that the -νθος place-names of Greek form a coherent group and so do the -σσος place-names. Whatever explanation we provide of the presence of one -νθος place-name both in Greece and Anatolia, it is likely to apply also to the other -νθος place-names, if we exclude of course later processes of extension and diffusion; similarly for the -σσος place-names. Yet, Θῆβαι and Μίλητος do not form a coherent group. The presence of the name Θῆβαι in two areas may be due to chance, or may be due to the way in which the Greeks reproduced a 'barbaric' place-name of Asia Minor, or may be due to an import from Asia Minor into Greece or from Greece into Asia Minor (and this may have happened in the prehistoric or in the historical period.) *One of the same explanations* will account for the presence of Μίλητος in Greece and in Ionia, but it does not follow that *one and the same explanation* accounts for both Θῆβαι and Μίλητος. It also does not follow that *one and the same explanation* accounts for Θῆβαι, Μίλητος and Κόρινθος.[28] In other words

[28] The same problem also arises when we consider the much larger list of Greek place-names compiled by Gindin (1967); cf. also Katičić 1976: 1, 91; Birnbaum 1974: 376. He aims at identifying roots (rather than suffixes) which can be compared with Anatolian roots in order to establish the existence of an Anatolian substratum in Greece. Here the missing link could be found if it were true that all roots considered are formally similar not merely to Anatolian roots but, more specifically, to Indo-European roots of Anatolian; yet, this does not seem to be the case. Moreover, one cannot forget that the roots are compared on purely formal grounds, since no meanings are available, and that the Anatolian material largely consists of names extracted from cuneiform sources where in most cases the spelling does not indicate distinctions (such as that between voiced and voiceless consonants) which are basic in Greek.

the lists produced by Haley and Blegen are not coherent; they may all represent pre-Greek place-names, but nothing guarantees that they are pre-Greek place-names which belong to the same language and that their presence both in Greece and in Asia Minor must be explained in the same way for all of them. If so, the conclusion may also be unacceptable. The problem would be different if Blegen and Haley had been concerned with the -νθος and -σσος place-names only, or at any rate with place-names linked by some formal factor, such as well defined suffixes or an obvious preference for some syllabic pattern, such as that defined by Hester for the -νθ-, -ρν-, -μν- and -νδ- names. My own view is that Blegen's conclusions do not rest on any definitive proof.

4.4.4. More recently, attention has concentrated on the place-names formed with the -νθος and -σσος/-ττος suffixes. Two reasons have contributed to it. On the one hand the need to deal with a coherent set of names rather than with the mixed bag considered by Haley and Blegen, on the other hand the increased awareness of the link between Greek -νθος and Anatolian *-nda*, Greek -σσος and Anatolian -(*s*)*sa*. In their turn the two Anatolian suffixes have come in for closer analysis, and in a series of articles Laroche has argued that (a) in the second millennium -(*s*)*sa*, -(*a*)*nda* and *-wanda* place-names occur in the whole of the Hittite Empire, and (b) there is no reason to assume that these suffixes were borrowed from a non-Indo-European substratum since they are perfectly at home in the Indo-European languages of Anatolia. -(*s*)*sa* may be taken as the neuter plural of the genetival suffix -(*s*)*sa*/*i*-, Luwian *-assi-* (perhaps from IE **-osyo*); -(*a*)*nda* and *-wanda* are connected with IE **-nt-* (and the omnipresent *-nt-* suffix of Hittite and Luwian) and with IE **-went-*.

In his list of the relevant place-names, Laroche stressed that although the suffixes are at home in the Indo-European of Anatolia, they are added indifferently to Indo-European and non-Indo-European roots, and that it is not possible to distinguish between Hittite and Luwian place-names when these suffixes are involved. In one of his early articles (1961) he also concluded that if the -(*a*)*nda* and *-ssa* place-names were to be connected with the -νθος and -σσος forms of Greek, this would give some support to the suggestion that in Greece Greek was preceded by another Indo-European language. Yet, at no stage did Laroche state that the theory of a pre-Greek Indo-European substratum could be actually demonstrated, and most recently (1977: 217) his tone seems to be far more sceptical.[29]

Other scholars, but not Laroche, have gone a step further and argued that there was a pre-Greek Indo-European substratum in Greece and that this was a Luwian

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[29] See mainly Laroche 1957; 1961; 1972; 1977.

substratum. The suggestion is based on linguistic, archaeological and mythological data, and has been both violently defended and violently attacked. I shall be concerned only with the linguistic evidence; this calls for an analysis of the data collected by Palmer, who has gone more deeply than the others in the linguistic side of the problem.[30] Palmer's argument (or rather the linguistic part of it) is founded on (a) the study of the place-names and their suffixes, (b) the analysis of another Greek suffix which he takes to be of Luwian origin, and (c) the suggestion that there are clear Luwian elements in the Linear A texts, if we read them attributing to the signs the Linear B values. I shall discuss these points one after the other before I turn to some more general considerations about Luwian and Hittite. First, the place-names. Palmer has pointed out that the connections between Greek and Anatolian place-names go beyond the identity of suffixes and extend to cover one place-name which is fully analysable in Anatolian terms. The Greek Παρνασσός matches the Anatolian *Parna(s)sa* which is built with the *-(s)sa* suffix on the root *parn-* of the Hittite and Luwian word for 'house'. Palmer argues that if *Parna(s)sa* 'makes sense' both in Hittite and in Luwian, and if it can be shown, as Laroche has demonstrated, that *parn-* is a typically Anatolian root, Greek Παρνασσός *must* be taken from Anatolian. What is true of Παρνασσός must also be true of the other -σσος names. *A priori* the Anatolian and Greek connections could be explained either by assuming a migration from Anatolia to Greece, or by assuming a migration from Greece to Anatolia, or by assuming a migration which from a third place moved both to Greece and to Anatolia. The existence of Παρνασσός speaks for the first hypothesis.

Secondly, Palmer stresses the importance of the names Δίκτυννα and Δελφύνη. As others have done before him, he connects Δίκτυννα with the name of Δίκτη (now attested in the Mycenaean *di-ka-ta-de*: *Diktan-de*) and with the suffix *-wa(n)ni-* which, in this form, is a typical Luwian suffix used to form ethnics. In Luwian *-wa(n)ni-* may be 'simplified' to *-u(n)ni-* and it is this form which Palmer compares with the -υννα of Δίκτυννα and the -υνη of Δελφύνη (formed on Δελφοί). The point is meant to show that these suffixes of Luwian origin were productive in Greek territory, i.e. that they did not only exist in words which could be borrowed as a whole or imported without understanding their meaning.

Thirdly, Linear A. Palmer suggests that a recurrent series of signs in what has come to be called the libation formula (which now occurs in a number of different texts in Crete), must be read as *ja-sa-sa-ra-ma*, *ja-sa-sa-ra-me*, *a-sa-sa-ra[*, *a-sa-sa-ra-me* and interpreted as 'Ma-donna', 'to my (*ma/me*) lady (*asasara-*)'. The Hittite word for

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[30] See especially Palmer 1958; 1965: 321-57; 1968; cf. also Palmer 1980: 3-26. For some objections see Mylonas 1962; Ruijgh and Houwink ten Kate 1962; Schachermeyr 1962; Hart 1969.

'lady' is *išḫaššara-* and in correspondence with Hittite *i* we often find Luwian *a*. In one of the 'libation formulae' Palmer also finds an Anatolian verb *tanu-*, a third person singular ending *-ti* (cf. Luwian *-ti*), a negative *na* (cf. Hieroglyphic Luwian *na*), a verbal form *da-* or *da-waḷ* 'take' (cf. *ta-* 'take' in Hittite and Luwian), a noun *du-wa-na* (cf. the verb *tuwa-* 'put' in Cuneiform and Hieroglyphic Luwian) and a pronominal element *i-ja-ḷ* (cf. Hieroglyphic Luwian *ī-* 'this'). He concludes that the connections with Luwian are too many to be due to chance. Linear A was used for a language similar to Luwian.

I shall neglect here the archaeological, historical and mythological evidence adduced and I shall just mention Palmer's conclusions. At some stage at the end of the Early Bronze Age some Luwian tribes migrated from Anatolia and settled in Greece. From the mainland they moved to Crete. Finally at a later stage (perhaps at the break between MH and LH) new Indo-European tribes arrived into Greece and replaced Luwian with their language, i.e. the language which at a later stage we call Greek.

4.4.4.1. So much for the theory and the linguistic evidence. I cannot discuss the whole problem, but something may be said about the linguistic data and their background.

It is now clear that in the second millennium Anatolia knew three closely related Indo-European languages: Palaic, Hittite and Luwian. They are first attested indirectly in names quoted in the Assyrian texts of Kültepe, probably to be dated to the nineteenth century BC. Yet it is extremely uncertain whether these texts contain names which can be defined as Luwian, as contrasted with Hittite. The most likely conclusion is that the texts contain names which match later Hittite and Luwian names, but this does not prove that at that time the distinction was meaningful. There are lexical elements which seem to be more definitely Hittite. In the case of Luwian, Laroche (1966 (i): 315) concludes quite correctly that '*aucun type kanisien ne correspond à un morphème spécifiquement louvite*;' he had previously explained (ibid.): '*le mot 'louvite' n'est plus aujourd'hui l'étiquette commode d'il y a trente ans, recouvrant la somme de nos ignorances.*' In other words, the Kültepe texts tell us little about Luwian.[31] The first Hittite texts go back to the eighteenth century at the earliest, the Luwian texts belong to the second part of the second millennium. In that period we think of speakers of Luwian as settled in the southern part of Anatolia. In the first millennium we have a dialect closely related to Luwian written in the so-called Hieroglyphic script in Syria, Cappadocia and Cilicia; the bulk of the texts dates

[31] For the evidence from Kültepe see especially Garelli 1963: 127 (with a summary of the previous discussions).

from the tenth to the eighth century. A third language, Lycian, written alphabetically, also belongs to the Luwian group, but the main documents belong to the middle or second half of the first millennium BC and continue until the fourth century or later. Luwian is a linguistic concept and we ought to stress that there is no clear evidence for Luwian as such before the second part of the second millennium. Mellaart (1971(1): 408; 1971(2): 703) wants to find Luwians at Kültepe, but we have seen that the evidence is not clear. He also speaks of a Hieroglyphic Luwian seal from level VI (*ca.* 2000-1900 BC) at Beycesultan, but this is an old idea of Bossert which must be rejected. It is not clear that the seal bears the signs of any script, or that the script, if it is there, is Hieroglyphic, or that Hieroglyphic at that time was used for Luwian. Similarly, Mellaart's statement (1971 (1): 408) that a Luwian is mentioned in an obelisk set up at Byblos not later than the twenty-first century and written in Egyptian hieroglyphs, cannot be accepted without reservations. It is based on the fact that the name Kukun is similar to a much later Luwian name, Kukunni—but there is nothing specifically Luwian in either Kukun or Kukunni (the presence of the name David in a document would not prove the presence of Englishmen in the area where the document was found).

The conclusions is that we have no data which bear on Luwian in the Early Bronze Age. Languages are not created from nothing and, if Luwian existed in the second part of the second millennium, it must also have had an earlier phase, but we have no direct evidence for it—which means that we cannot localize it or define it linguistically. On general grounds the presumption is that early Luwian was closer to Hittite than the later form of the language.[32]

4.4.4.2. I now come back to the arguments for the presence of Luwians in Greece. From a purely linguistic point of view it seems to me that none of them, with the possible exception of the Linear A interpretations mentioned above, speaks for the presence of Luwians rather than of Anatolians in general in Greece. If we want to segment an -υννα/-υνη suffix in Δίκτυννα and Δελφύνη and compare it with Luwian *-wan(n)i-*, we ought to remember that the Luwian form belongs to the end of the second millennium and not to the beginning of it, i.e. to the date of the supposed Luwian invasion of Greece. At that date presumably the suffix was closer to **-uwan-*, the Common Anatolian form (cf. Laroche 1960: 170-71) than to *-wan(n)i-*. It would be possible to derive -υννα/-υνη from a 'hellenized' form of **-uwan-* but, if so, the suffix does not point to Luwian but to Anatolian in general. Indeed, those who want

[32] Carruba (1969) has argued that we have evidence for Anatolian (not necessarily for Luwian) at an earlier period than generally supposed, but some of his evidence is doubtful and the conclusion has not been generally accepted. In any case the time extension gained would not be great.

to make use of the same Anatolian suffix to explain both -υννα/-υνη and the -υμνα place-names (Δαρυμνα, etc.) could probably argue that the original *-*uwan*- was differently treated in different parts of Greece or different dialects; it would be easier to do so than to argue that -υμνα is Hittite and -υννα/-υνη Luwian.

If this is so and if the rest of the evidence points to Anatolian, rather than to Luwian, is it Linear A that induces us to think of Luwian? Even this is doubtful. Palmer has analysed only one text, a libation table from Knossos (I 8 Brice; see now Godart and Olivier 1976- vol. 4: KN Za10), which he reads (1968: 348)

ta-nu-ạ-ti ja-sa-sa-ra-ma
na da-wa-[ ]du-wa-ṇạ i-jạ[

and tentatively translates (1968: 349-50)

[This altar X] sets up for My Lady
do not take the offerings from this (here) ...

If we assume for a moment that this interpretation is correct (cf. for a slightly different version Palmer 1965: 334-39), what is Hittite and what is Luwian? The *-sara-* suffix is Hittite; Cuneiform Luwian has *-sari(ya)-* and Hieroglyphic Luwian *-sa + ra/i-* with the usual *-i-* thematization. If this postdates the Early Bronze period, we could say that at an early stage *-sara-* was both Luwian and Hittite.

Palmer compares *a-sa-* or *ja-sa-* with Hittite *išha-* 'lord' and argues that the Luwian form would have been **ašha-*. However, a) neither **ašha-* nor *išha-* are attested in Luwian, b) not all Hittite *i*'s correspond to Luwian *a*, and c) I do not see what favours this interpretation over that of Carruba's (1968: 934), who compares the word with the attested *hašušara-* 'Queen', which is both Hittite and Luwian (except for the final vowel). Some phonetic assumptions are necessary for both interpretations.

We have evidence in Hittite for an enclitic possessive pronoun *-miš*; we have no such evidence for Hieroglyphic Luwian, while it is possible, but unlikely, that Cuneiform Luwian had an enclitic possessive pronoun of third person. It is conceivable that these enclitic forms were common Anatolian.

The reading *ta-nu-ạ-ti* is very disputed. Raison and Pope now read *ta-nu-ṭạ2-ti*. Luwian has indeed a *-ti* ending, while Hittite has *-zi*, but *-zi* derives from *-ti*, so that if *-ti* were attested in Crete it could represent an Anatolian archaism preserved in a lateral area; it does not speak for Luwian.

The identification of a monosyllabic *na* element at the beginning of the second line is disputed (cf. Pope 1961; Palmer 1968: 348). If it is acceptable, we should notice that *na* as a factual negative is presupposed both by Hittite *natta* and Cuneiform Luwian *nauwa*; *na* 'not' is attested in Hieroglyphic Luwian, but it seems incongruous to use linguistic material of the first millennium when we have Luwian data for the

second millennium in the cuneiform texts. More important, we now know (Hawkins 1975) that in Cuneiform Luwian the prohibitive negative is *niš* and in Hieroglyphic Luwian *ni*, and not *na* as used to be thought.

ta- 'take' is common to all Indo-European languages of Anatolia. *tu-wa-* 'put' belongs to Cuneiform Luwian, Hieroglyphic Luwian and Lycian. It is the most specifically Luwian form identified by Palmer, but Hart (1969: 167) has pointed out that the word may well have been [·]-*du-wa-ṇa*, which would make the identification impossible.

i-ja[can no longer be taken as a Luwian demonstrative on the basis of Hieroglyphic Luwian, since this has been shown to have a *za-* demonstrative like Cuneiform Luwian (*ī-* was an earlier interpretation).

The conclusion is that, even if we accept most of Palmer's interpretations of the words in the text, this has no specifically Luwian forms except for *]du-wa-ṇa*, and the reading of this sequence is doubtful. The bulk of the evidence, if accepted, would point towards Anatolian rather than towards Luwian.

4.4.4.3. How acceptable is the evidence as a whole? Let us say from the start that some of the points made about place-names are impressive. I am convinced that Laroche is right in attributing to Anatolian -(*s*)*sa* and -*nda* place-names and is also probably right (I am less certain about this) in assuming that the suffixes are of Indo-European origin. The complications arise *à propos* of the problem which Laroche has never discussed in detail: the identification of these names with the Greek names in -σσος and -νθος. In a few cases (*Parna*(*s*)*sa*) the identification is difficult to resist but we should remember Laroche's dictum (1977: 217): "*la resemblance du hittite* Parnassa *et du grec* Parnassos *ne suffit pas pour affirmer la présence de Louvites à Delphes*". Also, we must realise that, as has often been pointed out, the Greek category of -σσός names is not unitary. There are names which end in accented -σσος ('Ιασσος/'Ιασός) and names which end in unaccented -σσος (Κρίμισσος/Κρίμισος), there are names which end in -σσος and names which end in -σος (though it is not always easy to distinguish the two categories), there are -σσος names which have -ττος equivalents in Attic ('Υμεττός) and -σσος names which do not ('Ιλι(σ)σός). This is well known, but the result is that we gain the impression (confirmed by some of Hester's findings on syllabic patterns) that we cannot compare the -σσος names *in toto* with the -(*s*)*sa* names of Anatolia; the comparison is justified for a few names only. If so, there may be more than one explanation and we do not need to assume the existence of Anatolian settlements in Greek on the strength of the sheer immensity of the data. Moreover there is the perennial question of the -σσος/-ττος alternation. This points to an earlier form of the *-*tsos* or *-*ts'os* type; a simple *-*syos* or *-*s'os* would be more

likely to yield -σος (??) or -ιος in the whole of Greek. Yet, the Anatolian form, if Indo-European, must have been either *-s'a* or *-s(s)a*; why should it have been borrowed with an affricate?[33] If we exclude all the -σσος/-ττος names from our list, the comparison with Anatolian becomes less and less impressive. Once more we ask whether the normal process of borrowing and diffusion would not be sufficient to explain what is left.

What about -νθος? Here there is again a phonetic problem. Why would Greek have absorbed an Anatolian *-nd-/-nt-* in the form -νθ-? Borrowings are notoriously unpredictable in their phonetic behaviour—all the more so if we know little about the phonologies of the giving and receiving languages. Yet, we could reasonably expect -ντ- or -νδ- in Greek, especially if we have, as we seem to, an early Anatolian borrowing into Greek, the work ἔλεφας, ἐλέφαντος 'ivory', where, if Laroche (1965) is right, -ντ- reflects an Anatolian *-nt-* extension, i.e. the same suffix which Laroche finds in the place-names.

Two words like Δίκτυννα and Δελφύνη do not provide much in the way of evidence for a suffix -υννα/-υνη to be compared with Anatolian. Also, even if we allow the comparison to stand, as we have seen this leads us back to a Common Anatolian **-uwan-*, which is at the origin of the Hittite and Luwian forms. But there is no way of showing that this suffix is both Anatolian *and* Indo-European. If so, its presence in Greek, even if accepted, does not necessarily point to Anatolian influence. We cannot exclude a common pre-Indo-European substratum.

I finally come again to Linear A. It has often been said—and it may be repeated here—that one single text does not offer much evidence. Let us also remember that the readings are unreliable for two reasons: a) it is far from certain that in all cases we can use Linear B values for Linear A signs, and b) the readings of this particular text are doubtful, since a number of signs are scarcely recognisable. It can be added that the first word, which Palmer reads *ta-nu-ạ-ti*, begins with two signs which also appear in a word of the parallel dedication formula written on a silver pin from Platanos recently published (Alexiou and Brice 1976; Godart and Olivier 1976-, vol. 4: PL Zf1). If transcribed according to the same criteria the Platanos word reads *ta-nu-ni-ki-na*, which does not sound like a recognisable verbal or nominal form of Anatolian. Here too the conclusion is '*non liquet*'.

To sum up: from a purely linguistic point of view I can only say two things. First, I see no *linguistic* reason for formulating a 'Luwian' hypothesis, as contrasted with

[33] For the different types of -σσος names see e.g. Hainsworth 1969; 1972; Chadwick 1969: 86-93. For general reflexions on Greek place-names in the light of the recent evidence cf. Hainsworth 1972 and Schachermeyr 1972. The point about -σσος and -ττος has often been made and was discussed in detail by Wyatt (1968, with the earlier references).

an 'Anatolian' hypothesis. Secondly, however formulated, the Anatolian hypothesis is not yet supported by sufficient evidence. It cannot be excluded, but cannot be demonstrated.

5. My general conclusion is that, although there is in Greek a great deal of linguistic material which is not Indo-European or at least is not directly descended from Indo-European through the normal sound changes, we are still incapable of identifying the sources of this material and, above all, we are not in a position to say when and how it was acquired. If so, linguistics has little to contribute to the historical problems of the Early Bronze Age. The answer to the question which appears in the title of this paper is almost entirely negative.

5.1. Yet, I do not want to conclude on an entirely negative note. If no old problem can be solved, new ones can be created. What follows may complicate the question even further.

In 1967 Ruijgh (1967: 258-62), after an analysis of Mycenaean word-formation, concluded that in addition to the normal suffixes which Greek had inherited from Indo-European, Mycenaean had a suffix which, in spite of its deceptive appearance, was not likely to be Indo-European. The suffix appears in the form *-e-jo*, *-e-ja* (feminine), and perhaps *-e-i-jo*; it is excluded that it can be compared with Greek forms like Σωκράτειος, Ἀχίλλειος etc., since in Mycenaean these would appear as (regular) *-e-i-jo* (< **-esios*) and *-e-wi-jo* (< **-ēwios*) forms. On the other hand, the suffix cannot be compared with Indo-European **-eyos*, which was used to form adjectives of material and was continued e.g. in Greek ἀργύρεος, ἀργυροῦς. The meaning of Mycenaean *-e-jo*, *-e-ja* is that of a possessive suffix and is used to form adjectives with a possessive meaning; often it is added to *-a-* stems, but the final *-a-* of the stem is deleted before the suffix. Ruijgh concluded that the suffix was likely to be pre-Greek. His suggestion did not meet with general approval, but some recently published evidence from Thebes and Tiryns seems to show that the suffix is indeed a feature of Mycenaean and behaves in such a way that it cannot be identified with any Greek suffix of Indo-European origin.[34] This is not particularly exciting in itself, since it simply adds another element to our list of Greek forms without an Indo-European etymology. More exciting is the observation that perhaps we can localize the source of this suffix. Earlier on in this paper I have expressed some doubts about

[34] For the form and function of some of these *-e-ja* derivatives, especially in Thebes, see Killen 1979: 176; Morpurgo Davies 1979: 100-01, n. 52. Killen (ibid.) points out that one of the new texts from Tiryns has added yet another *-e-ja* adjective to the list. The same conclusion about the *e-jo* suffix and the Linear A evidence is reached independently by Killen 1983: especially 91. This article also offers the most detailed discussion of the Linear B suffix.

the wisdom of using Linear B values for Linear A signs. In some instances, however, this may be justifiable, especially if Linear A has sequences of signs which occur in the same order in Linear B and which in Linear B can be identified with words (such as Cretan place-names) which are also likely to occur in Linear A texts. Now, in Linear B we have evidence for a Cretan place-name, mentioned in the Knossos tablets: *su-ki-ri-ta*. The same sign-sequence also occurs in a Linear A seal from Phaistos (PH 37) and the sequence is long enough to make us think that the coincidence is not accidental. The first three signs are found in a graffito on a pithos from Ayia Triadha (II 7 Brice; Godart and Olivier 1976-, vol. 4: HT Zb 158b) where they are followed by three other signs. The last three signs can be read because they occur in other equivalent sign-groups of Linear B and Linear A. If so, our second word can be read (in Linear A) *su-ki-ri-te-i-ja* and compared with the place-name *su-ki-ri-ta* of Linear B and Linear A. Godart (1976: 42-43), who argues for this, points out that the contrast *su-ki-ri-ta*, *su-ki-ri-te-i-ja* gives us the first clear indication of a Linear A suffix. Perhaps we can go further. One of the characteristics of the *-e-jo*, *-e-ja* suffix recognised as pre-Greek by Ruijgh is that it is often added to *-a-* stems, but the *-a-* is deleted (Linear B *pe-ri-qo-ta* / *pe-ri-qo-te-jo*, *e-se-re-a*$_2$ / *e-se-re-e-jo*, *pa-ke-ta* / *pa-ke-te-ja*, etc.). Ruijgh has also argued that perhaps there are traces of this suffix in some Greek ethnics (Μινύειος), river names ('Αλφειός), etc. At this stage it may seem reasonable, in view of the similarity of form, function, and usage, to compare the Linear B suffix and the *-e-i-ja* suffix of Linear A, even if we have only one piece of evidence for the latter. Obviously it is impossible to make a conclusive case for the identification of the two suffixes on the evidence available; a definitive proof must wait for further evidence from Linear A.

We are allowed to speculate. If the identification were correct would this mean that Mycenaean influenced the Cretan language? This seems unlikely, all the more so because in Linear A we would be dealing with a Cretan place-name. Presumably the language of Linear A influenced Greek. But we must remember that the suffix is found in Linear B outside Crete too; it occurs in Thebes, for instance. Presumably the suffix somehow came to belong to the Mycenaean language, and from there may have been transmitted to later Greek. We do expect to find lexical borrowings from Minoan into Greek; the borrowing of suffixes is a less common event. At this stage I have only a series of questions to ask and I shall not try to provide any answer. Presumably if the mainland and Crete spoke the same language, before this was 'replaced' by Greek, the existence (if real) of a common suffix in the language of Linear A and in Mycenaean Greek would cause no problem. The suffix could have been borrowed anywhere and at any stage after the arrival of the Indo-Europeans into Greece. On the other hand, it is often argued that it is unlikely that Crete and

the mainland had a common linguistic background in view of the endless traces of disturbance in the mainland which did not affect Crete. But can we suppose that by the Mycenaean times a purely Cretan suffix had already influenced Greek so much that it became part of the normal Mycenaean language? This is not impossible of course, but may also have some historical implications. I leave the problem open.[35]

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[35] I owe a great deal to the participants at the Symposium where this paper was first read and in particular to E. L. Bennett, Jr. who read a first draft of the final version. Obviously, he is not responsible for any of the views expressed here, but I am certain that he would like me to point out, once again, that my suggestion about the identification of Linear B *-e-ja* and Linear A *-e-i-ja* is far too rash to be in step with the rest of the paper.

The manuscript of this paper was completed in the summer of 1979. After that it has only been possible to add a few general references.

WHO WERE THE IMMIGRANTS?

MICHAEL SAKELLARIOU

The question I have undertaken to treat within the scope of this colloquium on the end of the Early Helladic period, after the problem "Evidence for Invasions" which our friend S. Hood has chosen to illuminate, has indeed already been approached, but indirectly and from a different angle: as the problem regarding the time of the arrival of the Proto-Greeks in Greece. In point of fact, one of the three different answers proposed to this problem places the arrival of the Proto-Greeks at the end of the EBA, while the other two date it later: the second, around the transition from the MBA to the LBA; and the third, at the end of the LBA. Thus, without its having been explicitly stated, the question "Who were the immigrants at the end of EH?", has already received two answers: the first, corresponding to the first answer to the preceding problem, is "The Proto-Greeks"; the second, implicit in both the other answers to the same problem, is "Not the Proto-Greeks". Consequently, discussion of the new problem "Who were the immigrants at the end of EH?" should properly include a whole discussion of the old one, "When did the Proto-Greeks migrate to Greece?". Nevertheless, I shall not concern myself here with an account and critique of the answers that have been given to the older question, because it would require much more time than I have at my disposal. It is sufficient for me to present briefly a new approach to both questions, the old and the new. The facts and ideas I am now going to present are fully developed in my book *Les Proto-Grecs* (1981). In the book there is also a broad exposition and discussion of the views that have so far been proposed concerning the date of the arrival of the Proto-Greeks in Greece.

The new approach makes use of the results of three parallel studies, each of which is concerned with a different kind of evidence. They all have one purpose, however: to distinguish those elements which can throw some light on the identity of the immigrants at the end of the EH period.

A. *Archaeological Evidence*

It is from the archaeological evidence that we become aware of the transition from EH to MH. Therefore, when we ask whether this transition reflects the arrival of immigrants, and try to identify them, it is only reasonable to examine the archaeological evidence first, before looking at other kinds.

The archaeological evidence consists of a large number of innovations in different fields. Some of them clearly came from other countries, while others came about inside Greece. Naturally, only the former can testify to the arrival of immigrants, indicate their geographical origin, and possibly give us some clues about their identity. Innovations of the second kind are, of course, no use in solving these problems, but they do contribute to an understanding of some of the effects produced by the arrival of immigrants and their meeting with the old settlers.

Nevertheless, the innovations of external origin are of varying value as indicators of the arrival of immigrants and, consequently, for the advancement of our knowledge about the other two problems.

The new funeral constructions and burial customs, as well as the new house-types, carry the greatest weight as evidence in this context. Human societies are very conservative in their practices regarding the dead. When, therefore, we see appearing in Greece intramural burials, tumulus burials, a new type of cist grave, pit graves, the placing of ochre in graves, and traces of animal hide beneath the remains of the corpse, it can be taken as certain that we have to do with innovations which have arrived in Greece not as an imported fashion, but as customs brought with them by immigrants. The same idea applies to the oval as well as to the apsidal houses, since they are in every respect inferior to the houses lived in by the inhabitants of Greece before their appearance.

At the other end of the scale come all the portable objects of foreign origin, which could have been brought as merchandise or booty, or have been made in Greece by local people imitating foreign originals. Such objects include various toilet articles, armaments, better quality pottery, and tools and ideas different from anything possessed by the inhabitants of Greece. The evidential value of these elements is nil when they come from levels dated earlier than the arrivals of immigrants. They can take on a certain value when they occur in levels preceded by signs of destruction, and rather more when they are found together with good indications of immigration from the same geographical region. In estimating their value as evidence one must also take into account their frequency of appearance.

An intermediate position is held by portable objects unlikely to have been brought in as merchandise or booty or made in imitation of foreign models. These include the coarser pots (in our case, corded ware), and possibly types of tools or weapons that would not have brought any significant improvement to the technical and military resources of the inhabitants of Greece at the end of the EBA/EH period. As with the previous group, their value as evidence increases or decreases according to the associated finds and their stratigraphic position before or after undoubted indications of the arrival of immigrants. Thus, it is difficult to attribute to newcomers

objects of this group found in levels predating traces of destruction and not accompanied by stronger evidence of the arrival of immigrants from the same point of origin.

The highest degree of probability is achieved when a *Kulturfront* exists; that is, when enough good pieces of evidence are found together on a single site or in a restricted area, and when this occurs simultaneously over a considerable geographical region.

After taking into consideration not only all available innovations of external origin appearing in Greece in the last stage of EH times and the beginning of MH, but also the methodological precautions set out above, we can draw this conclusion: various parts of Greece were penetrated at that time by newcomers bearing with them elements of a culture originating in the northern Balkans and of another culture at home in the Eurasian steppes and the Ukraine. Innovative features of Eurasian and Ukrainian origin, characteristic of the so-called Kurgan culture, are the tumuli, a type of cist grave different from the Aegean one, a type of pit grave, the use of ochre and hides in burials, the oval plan of houses, the cord ornamented pottery, maceheads, and hammerheaded pins. From the northern Balkans came the custom of burying inside settlements, the apsidal plan for houses, perforated hammer-axes, and a kind of small flask.

The main features of these two cultures (burial constructions and customs and plans of houses) have so far been attested at fifteen EH sites and at fifty three MH ones—with some exceptions—in a few small areas of eastern Greece, mainly in Argolis and in Attica. The phenomena which occurred later are widespread throughout the Greek peninsula and in some adjacent islands.

If one simply considers the distribution of the same main features in Greece, one can draw the conclusion that some sites were occupied by the bearers of the Kurgan culture, others by bearers of a Balkan culture, others by groups of both origins. But we must take into account a number of other facts as well. 1) The most important innovations of Kurgan origin (tumuli, cist graves, pit graves, various burial customs) occur exclusively in cemeteries, while the chief innovations of North Balkan origin (intramural burial, apsidal houses) are known only from settlements. It is worth noting, then, that a numbner of EBA or MBA sites in Greece are either cemeteries of still unknown settlements or settlements whose burial places have not yet been discovered or explored. 2) Intramural burials and apsidal houses are frequently destroyed by later building activities, especially when this is accompanied by levelling of the terrain. 3) Many intramural burials following a Balkan tradition are cists of Kurgan origin. All these facts contribute to show the irrelevance of the first conclusion; the last fact makes it clear besides that cist graves inside settlements

have been made by a single people having inherited a feature of the Kurgan culture and a feature of a Balkan culture.[1]

B. *Linguistic Evidence*

It is generally accepted that, when two or more geographically separated languages have certain types of features in common, this comes from the fact that at an earlier period they were contiguous for a considerable length of time. Nowadays, studies relying on such data produce surer results than formerly, because they have been freed from certain sources of error. The most important was the use of features inherited from a common ancestor. But archaisms do not prove special connections between related languages or dialects; such connections are indicated above all by common innovations, and to a lesser extent by common elections. A second source of error is failure to realise that two related languages or dialects may develop common innovations entirely fortuitously while not being contiguous. This possibility weakens the more common innovations or elections increase and are shared by a greater number of languages.

Bearing in mind these methodological considerations, one can make correct use of the features common to Greek and other Indo-European languages put down by Porzig (1954). Greek and Aryan (the common ancestor of Vedic and Avestan) share eight innovations and eight words, about the latter of which one cannot be sure whether they were innovations, archaisms or elections. The relationship between Greek and other Indo-European languages is on a lower level: four innovations connect Greek with Italic, and the same number with Slavic. Only two innovations link it with Germanic, and another two with Armenian; the latter, however, also approaches Greek in respect to eight words. In combinations of three Indo-European languages, first comes that of Greek with Aryan and Germanic, which share three innovations and three words. Greek and Aryan come also in a group of four languages including also Slavic and Baltic; the common innovations of this group are two. Thus, it is clear that Greek had a closer relationship to Aryan than to any other Indo-Europan language. From this observation it can be concluded that the Indo-European dialects ancestral to Proto-Greek and Proto-Aryan were formed in regions that were contiguous, and that their contact was maintained over a considerable period of time. The second part of this conclusion is further strengthened by the fact that some of the innovations common to both languages are very ancient

[1] For details and references concerning the archaeological evidence, see my book *Les Proto-Grecs* (1981).

and others quite recent (Porzig 1954: 160). Another observation is of importance in this context: the dialects that preceded Proto-Greek and Proto-Aryan belonged to a group of dialects which developed more innovations than the rest. Studies of contemporary languages and dialects have shown that innovations occur mainly in the centre of a linguistic continuum, while archaisms prevail on the edges. From this, too, it seems that the dialects ancestral to Proto-Greek and Proto-Aryan remained in contact over a long period of time, since their speakers would have separated only after the linguistically conservative Indo-Europeans left (Crossland 1957: 30-31; 1971 (1): 867; 1971 (2): 229, 231, 235; 1972: 50-51). The region where the dialects that developed into Proto-Greek and Proto-Aryan bordered on each other is with good reason placed north of the Black Sea (Porzig 1954: 161; Crossland 1971 (1): 849).

C. *The Earliest Ethnic Name of the Greeks*

The Greeks made their appearance on the historical scene with three ethnic names. Of these, the name Ἀχαιοί had originally designated a pre-Greek people (Sakellariou 1977: 231-32, 239-40). The name Ἀργεῖοι means literally the inhabitants of the plain where the city of Mycenae was situated. The third name, Δαναοί, has been the subject of many theories, according to which this name, too, was given to the Greeks at a later date. I believe that all these theories are without foundation and that, on the contrary, it can be shown that the name Δαναοί was brought by the Proto-Greeks from the same region where the Proto-Greek language was formed, somewhere to the north of the Black Sea.

Most of the theories that attribute the name Δαναοί to foreigners have one starting point in common: the ancient belief that Danaos and his daughters came from the East. The same belief is also the source of some of the arguments they share. They differ significantly, however, on points of detail as well as in the rest of their arguments.

Thus Holland (1928) maintained that the Danaans were a branch of the Danuna, known from Eastern texts, and that they migrated to Greece around the beginning of the fifteenth century. This dating was based on mistaken ideas attributing some Mycenaean features to Eastern origin.

Bérard (1952) saw in the Danaans a section of the Hyksos. One set of his arguments was drawn uncritically from Greek myths. Another set relied on similarities of names: of Δάναος with the Semitic word *dan* (judge); of Ἄπις, Ἔπαφος with the names *Apopi*, *Ap(h)ophis*, borne by certain Hyksos kings in Egypt; and others. But no facts were adduced to convince us that these similarities are not just

chance. On the other hand he ignored facts about Danaos and Apis which point in other directions, as we shall see farther on. Bérard also drew a parallel between Io and the Egyptian Isis and the Semitic Anat, because all three appeared in the form of cows, even though goddesses in the form of, or with characteristics of the cow are known in other religions, too, including those of Indo-European peoples. Finally, Bérard overrated the significance of the Egyptian artifacts that have been found in Mycenaean tombs, and attributed to an Egyptian origin various features of the Mycenaean civilization which specialists do not consider to be Egyptian.

Later, Astour (1965; 1967) put forward the theory that the Danaans were Semites from Syria. His basic arguments aimed at showing that the names of Danaos and the Danaans in Greece, of Dan and Daniel in the Old Testament, and of Danel in the epic *Aqht* of Ugarit, go back to the Semitic *dan* (judge). This etymology was based on the passage in the Old Testament "Dan will judge his people as one of the Tribes of Israel"; on a passage in *Aqht* in which Danel 'judges the case of the widow, adjudicates the cause of the fatherless"; and on the story about Danaos that he punished one of his daughters because she had not obeyed him. But the meaning of the passage about Dan is different (Jones 1975: 121), and Danaos's action does not confer on him the quality of a judge. Moreover, this argument does not take into account numerous other names containing *dan* (a theonym in the Vedic religion, an ethnic name in Avestan, and many river names of Indo-European origin in the Ukraine, in Greece and in the East). All Astour's other arguments, which are many, have a subsidiary purpose, to relate in other ways as well Danaos with Dan, Danel, Daniel, Moses; to demonstrate similarities between the Danaïdes and the sisters of Dan, between Io and Semitic divinities; to find traces of Moses in Greece; and so on. These arguments did not achieve their purpose because they are forced.

Yadin (1968) too brought together the Danaans, the Danuna and the Hebrew tribe of Dan, maintaining however that this tribe joined the Israelite Confederacy at a later date than the other tribes. In support of his first view the author cited the Greek myths which refer to the adventures of Perseus and Mopsos in the Eastern Mediterranean, and tried without success to show connections between Sampson, hero of the Dan tribe, and the Greek heroes Heracles, Perseus and Mopsos, who belong to the Danaans. His second view does not concern us here.

Both Yadin's views have been further developed by Jones (1975). According to him, the Danaans were Semites who came to Greece from Lower Egypt around 1500 BC; they acquired the name Δαναοί in Greece. Some centuries later they took part in the attacks of the Sea-peoples on Egypt. Finally, they were accepted into the Israelite Confederacy. We will consider only the arguments by which Jones seeks to prove that the Danaans were Semites: 1) he traces the etymology of the name *Abas*,

borne by a grandson of Danaos, to the Semitic *aba* (father), without, however, showing that a similar meaning attached to this name; 2) he relates Danaë to Dam-Kina, a Summerian lunar deity, also without showing that the name Δανάη meant "moon"; 3) he identifies Perseus with Moses because both of them were said to have been saved in a floating chest, although this mythological theme is very widespread. As we have already remarked about a similar case, Jones's theory of a Semitic descent for the Danaans also suffers from his failure to take into account the numerous names of Indo-European origin containing *dan*. The same author's hypothesis that the Danaans came from Egypt is based on ancient Greek myths about the origin of Danaos and the adventures of Io and Perseus, which he used without critical caution, as well as on the view that the discovery of springs by Danaos and his daughters reflects a knowledge of water-control techniques that the Danaans could only have brought from Egypt. Jones's view that the name Δαναοί was created in Greece rests on equally shaky arguments. The other points in his theory do not concern us here.

Other scholars have connected the Danaans with Indo-European groups: Kretschmer with the Scythians, and Cook with the Celts. Kretschmer (1935; 1940; 1950 (1); 1950 (2)) supported his view with the following arguments. (A) The name Δαναοί is to be compared first, with Vedic and the Avestan *danu*, Ossetic *don* (river); second, with the names of rivers Δανούβιος / *Danuvius*, Τάναϊς / *Don*, *Danapris/Dnjepr*, *Danastris/Dnjestr*, Tanew, Τάναος; third, with *Danu*, *Danawa*, names of Vedic deities related to water; fourth, with the ethnic name *Danu*, plur. *Danawo*, cited in the *Avesta*; fifth, with Scythian personal names Tan(a)us, *Tanusa*; and sixth, with the surname *Tanaoxares*, given by Cyrus the Great to his son, Smerdis. (B) The toponym Σκυθία associated with Rhodes and Delos must go back to settlements by Scythians, who also must have been bearers of the ethnic name Δαναοί. The other parts of Kretschmer's theory need not concern us here. Kretschmer opened the way to the view that the Danaoi were Indo-Europeans, but he failed to prove that the similarity of the names Δάναος, Δαναοί with the words and names given above is not simply fortuitous. Moreover, the arguments by which he wanted to show that the Danaans were, more specifically, Scythians, are inconclusive. In fact, first, we do not know if the river names in the Ukraine containing *dan* are of Scythian origin; second, Scythian personal names with the same element in them only show that the Scythians, too, shared the isogloss of the root *dan* and of its derivatives; third, Kretschmer did not prove that the toponym Σκυθία came to the Aegean with the Danaans.

Cook (1940: 362-70) linked the name Danaoi not only with the names of gods and rivers suggested by Kretschmer but with others as well: Ἀπιδανός, Ἠριδανός, *Rhodanus*, Σάνδανος. He further noted certain connections of Danaos, the Danaïdes

and Danaë with water. But he related the Danaans with the Celts appealing to the theonym *Danann* in the old Irish religion, and to the location of the river *Rhodanus* in an area inhabited by Galatians. These arguments may show that the Celts, too, shared the root *dan*, but they are far from proving a special relation between Celts and Danaans, since similar names occur in so many Indo-European languages.

For all their deficiencies, the studies referred to have made important contributions to the advancement of the problem, directly, through the material they mobilized, and indirectly, by their instructive errors. To bring a solution to the question, all that remains, I believe, is for both etymologies, the Semitic and the Indo-European, of the names Δάναος, Δαναΐδες, Δανάη, Δαναοί, to be considered comparatively, not only from the phonetic point of view, but from the morphological and semantic as well.

In the names Δάναος, Δαναΐδες, Δανάη, Δαναοί, the vowel of *dan-* is short. The Semitic *dan* has a long *a*, but it becomes short before suffixes (Astour 1965; 1967: 46). In the Indo-European *dan-* the *a* is sometimes short and sometimes long. It is short in all the names of rivers located in Greece, or to which Greek names having a known prosody are ascribed; it is long in Aryan languages, but in them the long vowels are shorter than the long ones in Greek. Using the prosody of *dan*, *dan-*, then, one can relate the names Δαναός, Δαναΐδες, Δανάη, Δαναοί, either with the Semitic *dan* or with the Indo-European *danu*.

The etymologies of these names have not as yet been examined from the morphological point of view. In that respect it is worth noting the absolute correspondence of the fixed part of the Greek names under consideration, which was originally *danaw-*, with the Vedic *danaw-*, which is a derivative of the theonym *Danu*, and with the Avestan ethnic name *Danu*, plur. *Danawo*. Thus it seems that in all three languages, Greek, Vedic and Avestan, the stem *danaw-* can be broken up into *dan-*, the root, and *-aw-*, a suffix very usual in Indo-European languages. The Semitic etymology of the names we are discussing does not explain the *-aw-*. Consequently, the morphology accords only with the Indo-European etymology.

To confirm the derivation of the names Δάναος, Δαναΐδες, Δανάη, from the Semitic *dan* (judge), it is necessary to prove that the figures bearing these names had judging as their main attribute. As we have seen, the only attempt made in this direction, for Danaos, failed. For Danaë and the Danaïdes no attempt has even been undertaken. Therefore, the Semitic etymology of these names has not been proved. Let us now see if the connection of these names with the Indo-European *danu* (water,river), with the names of the water-gods in the *Veda*, and with many river names helps to supply a confirmation. It was believed that Danaos had transformed the Argolid from a poorly- to a well-watered country, discovering water supplies and

digging wells. Some of his daughters, the Danaïdes, are connected with springs. Furthermore, two of the themes in the myths about them, the beheading of the sons of Aigyptos and the burial of their heads in the Lerna lake-bed, have already been compared with the magic rites of pouring blood in order to make water gush forth (Welcker 1824: 400-401) and of using corpses' heads in order to bring rain (Gruppe 1906: 1209, n. 2; Cook 1940: 309). These are legitimate parallels. Thus it is very probable that this part of the Danaïdes' myth is an echo of memories of old human sacrifices that might take place on the edge of the Lerna lake and be followed by the burial of the heads in the lake-bed, in the hope of raising the water level when it had fallen considerably, or of making rain fall after a prolonged drought. The Danaïdes themselves before becoming mythological figures would probably have been either water-spirits to whom human sacrifices were made or members of a thiasos of women who performed the sacrifices. Why are the victims of the supposed sacrifices represented in the myth as the sons of Aigyptos? Aigyptos was called by the Greeks the river Nile in earlier times. It is then possible that the Danaans settled near the lake of Lerna sacrificed Egyptians with the idea that they were sons of the god of Nile, and in the hope that in this way the arid Argolid would acquire water. For the same purpose they can have made Danaos, to whom they attributed the watering of the Argolid, the brother of Aigyptos. Hence the belief would have arisen that Danaos and his daughters came from Egypt. Danaë also had associations with water. The hypothesis put forward by some scholars that in the older version of the Danaë myth Zeus came to her not in the form of a shower of gold, but as a shower of rain, is plausible because, according to an idea widely spread among primitive societies, water, and particularly rainwater, fertilizes both women and animals (Cook 1940: 452-53, 475-76). It seems, then, that Danaë personifies the women who, at certain festivals, sprinkled themselves with water in the hope of increasing their fertility, as well as that of the cattle and of the plants. Consequently an Indo-European etymology for the names Δάναος, Δαναΐδες, Δανάη is confirmed by the main role which, it appears, all these figures originally had. Such confirmation does not exist for the name Δαναοί; but it is natural, because this ethnic name is clearly not a primary, but a secondary one, deriving from the name of the god Danaos.

The confirmation of the Indo-European etymology of the names Δάναος, Δαναΐδες, Δανάη is not the only indication that the Danaans were Indo-Europeans. There are others: (1) The river names *Apidanos*, *Apidon* are compounds of *-dan-*, which we examined above, and *api-*, which has already been related to Indo-European *ap/ab* (water); (2) the name *Akrisios*, borne by Danaë's father, has been compared with the name *Akrisias*, which belonged to a Phrygian god; (3) the name of Proitos, brother of Akrisios, also appears in Phrygia; (4) the bilingual inscription at Karatepe, in

Cilicia, states that the king of the Dnnjm (in the Phoenician text), or Dnn (in the Hittite hieroglyphic text) belonged to the house of Mps (in the first text) or Mks (in the second); this ethnic name has been identified with the name Δαναοί, and the head of the royal house with Mopsos or Moxos, the hero of many Greek myths set in Greece and different countries of the East, including Cilicia. The rendering of this name sometimes with *p* and sometimes with *k* has rightly been thought to be the double result of the Indo-European labio-velar q^u.

There are many reasons, therefore, for supposing that the Danaans were Indo-Europeans. Since they were Indo-Europeans, they can have been Proto-Greeks. This conclusion is to be drawn from the use of the ethnic name Δαναοί in the Homeric epics. And it is indirectly reinforced by the very few phonetic phenomena to be observed in names associated with the Danaans. Thus, the preservation of the *k* in *Akrisios* (provided that the etymology of this name from the root *akr-* is correct) and of the labio-velar q^u in *Moq^{u}sos* (this is how this name still would have been pronounced at Karatepe in the eighth century BC) accords with Greek phonetics before the beginning of the first millennium BC, but not with Pelasgian (in which *k* underwent consonantal mutation, and q^u had been palatalized), Thracian (which also knew the consonantal mutation), Luvian or Hittite Hieroglyphic (which had labialized q^u), and Indo-Iranian (where both *k* and q^u have been assibilized). That the Danaans were different from Indo-Iranians is also apparent from the differences between the functions of Danaos, the Danaïded, Danaë and those of Danu, Danawa an ancient Indian religion. The later appear as demons who had stopped the cosmic water from flowing, while on the contrary Danaos, the Danaïdes and Danaë are associated with the provision of water and the life-giving properties of water. In other words, although a genetic relationship of both the Indo-Iranian and Danaan deities is virtually beyond doubt, each group of gods has developed on different lines. It seems, therefore, that the Danaans were not the same as the Indo-Iranians, or the Luvians, or the Thracians, or the Pelasgians, and this reinforces their identification as Proto-Greeks, a fact directly inferred from the Homeric epics.

Enough facts from the evidence we have used for the etymology of the names Δάναος, Δαναΐδες, Δανάη are in agreement to indicate that the Danaans came from a region north of the Black Sea: (1) The greatest concentration of river names with the stem *dan-* is to be found in the Ukraine and in Greece; (2) the same stem occurs mainly in the Aryan languages; (3) the stem *ap-* is known in Aryan languages and also in Illyrian and Germanic (in Latin and Celtic the variant *ab-* is found; in other Indo-European languages occur other variants); (4) a people of the name of *Danu*, plur. *Danawo*, is mentioned in the *Avesta* as being an enemy of the Iranians; in view of the obvious identity of this ethnic name with the ethnic name of the Danaans,

which originally had the form *Danawoi*, and of the fact that the Avestan *Danawo* were Indo-Europeans, it is permissible to speculate that the Greek Danawoi and the Asian Danawo were branches of one original people.

D. *Comparisons between Conclusions from Different Kinds of Evidence and Further Inferences*

(1) We have seen that the bearers of the name Δαναοί and the myths about Danaos, the Danaïdes, Danaë, Akrisios, Proitos, Perseus and Mopsos were the same as the bearers of the Proto-Greek language to Greece, or at least a large part of them.

(2) We have seen, too, that the Proto-Greek, the name of the Danaans, the connection of Danaos, Danaë and the Danaïdes with water, and some of the innovations that characterize the transition from EH to MH came to Greece from the Ukraine.

(3) To this we can now add the fact that innovations of the same category coincide with Danaan mythical figures and Danaan place names inside many Greek provinces, and occasionally within the even narrower limits of sites. We list these provinces together with the data we referred to:

Pelasgiotis: cist graves, corded ware, mace-heads ~ Danaë, Akrisios, Perseus, Mopsos;

Phocis: cist graves, corded ware, mace-heads ~ Akrisios;

Boeotia: tumuli, cist graves, corded ware ~ Danaë, Proitos, Mopsos;

Attica: tumuli, cist graves, mace-heads ~ Perseus, Mopsos, river Eridanos;

Argolis and Thyreatis: tumuli, cist graves, use of hide in burials, mace-heads ~ Danaos, Danaïdes, Danaë, Akrisios, Proitos, Perseus, river Tanaos;

Arcadia: cist graves ~ Apis, river Apidanos;

Messenia: tumuli, cist graves ~ Danaos;

Laconia: cist graves ~ Danaos.[2]

The Greek provinces in which either only external innovations or only traces of the Danaans appear are few, and the apparent absence in them of either the first or the latter can be set down to the gaps in our knowledge.

4) We have seen, furthermore, that Proto-Greek and Proto-Aryan lost contact with each other, and their speakers went off in different directions very close to the end of the break up of the Indo-European linguistic continuum. Here it would add that the break up of the Kurgan culture continuum took place at the time that corded ware was in its fourth phase, and that examples of this pottery reached Greece during the EBA/EH III period. Thus the arrival of the bearers of Proto-Greek in Greece

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[2] The same book deals at length also with the above-mentioned legendary figures and place-names.

coincides chronologically, too, with the arrival of bearers of the Kurgan culture in the same country.

5) From the above conclusion we may now infer that features of Kurgan culture occurring in Greece before the first appearance of corded ware IV cannot be attributed to the Proto-Greeks, but must have been brought in by other immigrants. This is particularly true with the tumuli of Lefkas, the most ancient of which go back to EH II.

6) Only now can we approach the matter we left open above: How did features of both a Balkan and Kurgan cultures come to mingle in an area somewhere between the Ukraine and Greece and to be introduced into the latter country by the same waves of immigrants? There are two possible answers. Either the Proto-Greeks, bearers of the Kurgan culture, upon entering the Balkans adopted features of a Balkan culture; or the same Proto-Greeks and some groups settled in the Balkans intermingled, with the result that their different cultural traditions merged to some degree and a new ethnic community arose. Let us examine the probability of each of these two hypotheses. We have said that human societies are very conservative in their burial customs. This constitutes an insuperable objection to the first hypothesis, but a weaker one to the second. The first hypothesis implies that people accustomed to bury their dead not only outside their settlements but also beneath artificial grave-barrows received from others the practice of depositing corpses in graves located inside the settlements, graves which lacked any distinguishing surface mark. This change in burial seems very radical, even retrograde, and it would remain inexplicable under the terms of the first hypothesis. The same hypothesis is further weakened by the fact that one encounters intramural as well as extramural burials, mostly in tumuli, at six sites. The fact that we do not find extramural burials in a greater number of sites showing intramural burials does not constitute a serious objection to the latter argument, given the accidental character of our archaeological evidence. On the contrary, the second hypothesis involves only the adoption of the cist grave by people persisting in intramural burial. So far as the apsidal houses are concerned, they might have been introduced to Greece either exclusively by the Balkan element in the immigrants or by both the Balkan and the Kurgan elements, the last having meanwhile adopted this plan of housing. It would be even more natural that both elements had mutually exchanged influences in the spheres of pottery, weaponry, tools and other implements. It would seem, therefore, that the immigrants we have been looking for were a mixed people composed mainly from Proto-Greeks originating in the area north of the Pontus, belonging to the bearers of the Kurgan culture, and named Danaans, and secondarily from an unknown people of Balkan origin. The question as to whether this last people spoke an Indo-European language or not must remain open.

7) Archaeological evidence shows us that during the EBA/EH III period, sites on or near the eastern seaboard of the Greek peninsula were taken over by immigrants; at the same time and in the same geographical region, other settlements were abandoned, most frequently after their destruction. The localization of these phenomena suggests that the newcomers crossed the Aegean setting out from ports on the northern shores of this sea; the Troad, too, may be included. From the beginning of the MBA/MH period onwards, the sites showing innovations of Balkan and Kurgan origin, or merely destruction and abandonment, occur over the whole of the Greek peninsula except in those areas where archaeological research is still limited. Did this spreading of immigrants take place from the earlier occupied sites on the east coast, or from some other region? The latter seems probable in view of the following facts: 1) The Ionians' name can be traced back to Histiaiotis in northwestern Thessaly; some Arcadian heroes are also found in western Macedonia; the Boeotians' name comes from the name of the Boion mountain in the Pindus range; the same range was the cradle of the Makednians, ancestors of both the Macedonians and of a part of the Dorians; the Thessalians came from Epirus;[3] 2) there is archaeological evidence of movements about the beginnings of the MBA/MH from Macedonia into Thessaly and from Thessaly into Boeotia (Hanschmann and Milojčić 1976: 101-103, 106, 154, 192, 194, 216-17, 220, 225, 228-29). Thus, with the aid of indications drawn up from two different fields of evidence, one can picture to a considerable extent both the main phases and the itinerary of the spread of the blend of Proto-Greeks and of some Balkan element in the Greek peninsula during the EBA/EH III and the MBA/MH.

[3] For details and references concerning the origin of the above-mentioned Greek *ethnē*, see another book of mine, *Ethnè et Dialectes Grecs à l'Age du Bronze*, to be published later.

THE EARLY BRONZE AGE IN WEST ANATOLIA

MACHTELD MELLINK

(Plate 16)

Anatolia has a considerable number of large mounds containing the remnants of fortified towns of the Early Bronze Age. The houses and fortifications of these towns were largely built of mudbrick which provided the substance for mound formation. Many of the central Anatolian towns of the Early Bronze Age continued into the second millennium, e.g. the sites of Kültepe-Kanesh, Alişar, Alaca Hüyük, Eskiyapar near Alaca, Acemhüyük (Burushanda?), and Karahüyük near Konya, sites measuring up to 650 × 600 m. for the central, fortified town exclusive of terraces containing suburbs. Such towns became centers of commerce and attracted the interest of Mesopotamian merchants perhaps as early as the Akkadian period (twenty-fourth to twenty-third centuries BC), certainly in the Old Assyrian period (twentieth to eighteenth centuries BC). At least by the beginning of the second millennium BC, in MB I, cuneiform records were kept in the Old Assyrian script and language in several central Anatolian towns, including Hattush, the site which was to become the Hittite capital.

Large mounds of this type also exist in East Anatolia and in the Syro-Cilician zone. Tarsus stands out as an excavated type-site; unexcavated towns of the third and second millennia BC are visible in the Cilician plain and in the Amuq; they grow in number and size along the trade routes to Mesopotamia. Anatolian archives are not yet available for the EB-MB trade between East and South East Anatolia and Mesopotamia or Syria, apart from references in texts from Kültepe, but ample contacts are attested by glyptic and ceramic data. We may assume that most of the leading towns East and South of Kanesh had administrative and commercial literacy by the early second millennium BC.

West Anatolia is not so easily considered part of this developing Near Eastern commercial and historical continuum. Large mounds become less numerous as one approaches the West coast (French 1969). Bronze Age mounds of urban size barely exist in the Troad, in Ionia, coastal Caria and Lycia. It is true that we have lost the prehistoric settlements where timber architecture prevailed over mudbrick, and changes in the watertable have submerged many prehistoric coastal sites. The relatively largest West Anatolian towns are found in the inland zone rather than as

harbor towns, although Old Smyrna-Bayrakli may have been a sizeable settlement in the Early Bronze Age.

Economic organization and prosperity of urban communities were probably not as developed in West Anatolia as in the center and East. One clue to the different developments—and to the different tempo in the adoption of writing—is the amount of contact with historical enterprise based in Mesopotamia (Mari, Assur) and Syria (Aleppo, Ebla). The West, although prosperous in fields and flocks, was initially too remote for intensive caravan trade and the development of regular stations of foreign commerce (Blegen 1956). The commodities sought by Near Easterners (e.g. copper, silver, horses) could be obtained through the network of Kanesh and affiliated centers, although indirect connections may have extended to the leading communities of West Anatolia as far as the Aegean coast.

The natural connections of the coastal zone of West Anatolia are with the Aegean area. The coastal Anatolians turned to navigation as their Aegean neighbors did; they came to know the island dwellers and traders better than they knew the faraway urban trade centers on the Anatolian plateau. West Anatolia by its dual exposure to East and West has the potential of becoming the Aegean's best link to history, but archaeological exploration has only begun in this area and has not nearly reached the intensity of field- and paper-work that has by now been generated by the Aegean world.

For the substance of West Anatolian culture in the Early Bronze Age we do not yet have a firm and objective sample. We note that in architecture, burial customs, and artefacts West Anatolia stands somewhat apart from the rest of Anatolia. Troy I and II show the presence of citadels with the so-called megaron as the preferred building type. The megaroid building unit also prevails at Poliochni on Lemnos and at Thermi on Lesbos, and it even seems to dominate at Beycesultan, well inland, in the EBA. Freestanding megara of simple form and small size make up the village of Karataş in northern Lycia during the EB II-III periods (Mellink 1967: 257-63; 1968: 243-53; Warner 1979). Villages and citadels with houses of this type have an appearance quite distinct from their contemporaries on the plateau (Alişar, Kültepe). Architecturally, West Anatolian communities of the Troy-Karataş type (at opposite ends of the economic scale, but representing the range from the Hellespont to the Mediterranean) have more affinity to European prehistoric villages with freestanding long houses than to Near Eastern irregularly clustered complex dwellings.

The burial customs of West Anatolia are unevenly known, largely because of the extramural nature of the cemeteries (Wheeler 1974: 415-18). The tombs of the Trojan rulers and commoners have not yet been found; large pithos cemeteries have

been excavated or looted in the Yortan region and all the way from Mysia to Pisidia (Bittel 1941). The village of Karataş had a conservative pithos cemetery along its outskirts. Excavated burials are also known from the Sardis area (Mitten 1971) and from Aphrodisias (Kadish 1969: 63-65). A blending of West Anatolian and Cycladic burial customs occurs in the cist graves of Iasos on the Carian coast (Levi 1962: 555-71; Pecorella 1977; Wheeler 1974: 418-19). In the extramural character of its cemeteries West Anatolia differs from what we know of central Anatolia in the Early Bronze Age; pithos burials are, however, a general trait (Özgüç 1948: 60-89). Burials of kings and prominent warriors may have been of special type in both West and central Anatolia, the most prominent being the shaft graves of Alaca Hüyük, but even the headman in Karataş had an unusual built tomb to himself (Mellink 1969: 324-27).

The artefacts found in the Early Bronze Age sites of West Anatolia give evidence of local fashions but also of increasing prosperity. The pottery especially in the earlier part of EB has such strong local traits that archaeologists have been tempted to subdivide West Anatolia into a considerable number of cultural zones. The strength of local customs of pottery making is probably due to the lack of intensive contact among districts; handmade pottery of the EB I villages, presumably made by village women largely for their own households, tended to remain conservative and did not travel far from its place of manufacture. In the course of EB II and III, changes occur, one of the principal innovations being the introduction of the wheel and the beginning of fashions in the making of popular pottery shapes, so that indeed ceramic classifications acquire more than parochial significance. While the wheel is imported through technical contacts with the East (Cilicia and Syria), contacts with the Aegean are also on the increase, and pottery begins to travel both because of its individual attraction or utility and as part of wholesale transmission of customs and interests.

In other categories of artefacts, we note a peculiar parochialism in early figurines (terracotta figurines of various types can be grouped with local EB I ceramic traditions), and the uneven distribution of other terracotta and clay objects (beads, spindlewhorls, simple stamp seals). In the category of seals, we see a considerable upsurge in EB II-III. The introduction of cylinder seals in central and South East Anatolia is noticeable especially in Cilicia, where pottery shows imprints of seals, some of which may have been stamp-cylinders (Garstang 1939: pl. 75, 17; pl. 81, 8), and where merchandise was sealed with clay impressed with designs of cylinders and stamps (Goldman 1956: figs. 397-98). The use of hybrid stamp-cylinders, which continues in the second millennium in the South of Anatolia, is also attested for Troy II by sealings on large jar fragments (Schmidt 1902: no. 2552). The most impressive

seal of this shape comes from Poliochni Yellow, Troy II period, with designs still related to Early Dynastic Mesopotamian iconography (Bernabò-Brea 1976: 298, pl. 254). Hoards of seal impressed bullae comparable to the find groups from Lerna III (Wiencke 1975) have not yet been found in West Anatolia; the presence of occasional bullae (e.g. at Karataş in EB II, Mellink 1972: pl. 55, 5) shows that the commercial custom of sealing merchandise was known. Stamp seals are the native seal type of West Anatolia, as proved by dozens of clay (rarely stone or metal) stamps from EB II Karataş. The cylinder-stamp is a sign of cosmopolitanism, like the introduction of the wheel. It is found at coastal sites (Mersin, Troy, Poliochni); a simplified version was found in a cist grave on Amorgos (Renfrew 1967: 18, no. 19; Buchholz and Karageorghis 1973: no. 1362).

Metallurgy is remarkably advanced and active in West Anatolia. We are used to the splendors of the treasures found by Schliemann in the burnt level of Troy II with their evidence for remarkable craftsmanship in the making of copper and bronze tools, weapons, and vessels; silver, electrum, and gold vessels, and a variety of personal ornaments in those precious metals. The period of Troy II is one of intensified contact with central Anatolia, as proved by remarkable resemblances between jewelry from Troy, Poliochni and Alaca Hüyük, even more so from neighboring Eskiyapar (Temizer 1977: 293). Overland trade develops, perhaps partly on West Anatolian initiative, and craftsmen travel with their tools and moulds to serve prosperous customers along a route which extends from the heart of Anatolia to the West coast (Canby 1965). The art of North-central Anatolia, viz. the composite metal figurines and standards found in the so-called royal tombs of Alaca Hüyük, does not travel West, and the religious iconography which begins to emerge in the pre-Hittite coreland is not characteristic of the West, which remains inarticulate (at least to us) in this respect.

Jewelry and weapons of West Anatolian type are known from various burial sites in the Yortan district, from Aphrodisias (Kadish 1969: 63), Sardis (Mitten and Yüğrüm 1971: figs. 4-7), and Karataş (Mellink 1969: pl. 74), with some regional variation, but in a quantity which indicates some material wealth even of the villagers and small townspeople in the middle and later part of the third millennium B.C.

If we ask for archaeological proof of contacts with the Aegean, we find clear ceramic indications in West Anatolia's second, more 'cosmopolitan' phase (EB II-III). We know that Poliochni stands close to Troy especially in its Yellow phase; we note interaction of West Anatolia with the Cyclades in pottery and metal work. The contacts with the Aegean deserve to be scrutinized not just as signs of increasing cultural and commercial interaction, but especially as potential clues to an inter-

pretation of the prehistoric vicissitudes of the lands around the Aegean during and at the end of the Early Bronze Age.

Neither in the Aegean nor in West Anatolia do we see a peaceful gradual development from prehistoric isolation through contacts and stimuli to proto-history and history. In both regions violent interruptions and changes take place. The life of West Anatolian communities is threatened as they gather wealth. Fortification walls of modest size (Karataş, Mellink 1974: 352; Demirci Hüyük, Korfmann 1979: 13) or monumental ambition (Troy I and II) are built around the sites of the privileged; in most instances to no avail since looting and conflagration mark the end of most prosperous establishments. We also note appearances and disappearances: new sites are built, grow and remain visible through their mounds or cemeteries, but many of them do not last into the second millennium. In West Anatolia, too, we must ask how the Early Bronze Age developed and ended and what kind of major prehistoric upheavals affected its course. The answers cannot come exclusively from the Eastern neighbors (central and South East Anatolia) nor from the Aegean, but both regions must have had a share and role in the fate of West Anatolia during the third millennium BC.

If we try to focus on details, we shall need a framework, a chart setting out the relative order of events on either side of the Aegean (Pl. 16). Many such charts have been designed by many of us, and West Anatolia is usually represented by its best known site, Troy, for which we have the extensive and admirable analyses by Schliemann, Dörpfeld, and the Cincinnati team. Troy remains our traditional center of interest and one of the best yardsticks for developments along the East coast of the Aegean. Yet, there is much debate concerning Trojan chronology and Troy's position in the various stages of the Aegean and Anatolian Early Bronze Age (Hood 1971; Spanos 1972; Easton 1976; Podzuweit 1979; Yakar 1979).

We can try to supplement the story of Troy as much as possible with the aid of other sites in West Anatolia and island sites to see how the major upheavals and incisions can be coordinated. We need to look at Cilicia also, since a Cilician connection with the Troad becomes obvious in the later third millennium BC.

It is not necessary to analyze the precise beginnings of Troy I. From Kumtepe (Sperling 1976), Fikirtepe (Özdoğan 1979) and Poliochni (Bernabò-Brea 1964) we know that there are earlier sites than Troy I in the general area and that the foundation of Troy does not mark the official beginning of the Early Bronze Age in West Anatolia. Coexisting and overlapping with Troy I we have Poliochni (Blue-Green-Red), Thermi I-V, Emporio on Chios, and many cemeteries, hence villages, of Yortan type in Mysia, a bit inland. We have evidence for habitation at Old Smyrna-Bayrakli (Akurgal 1950: 54) and other coastal sites (Clazomenae), none of them ex-

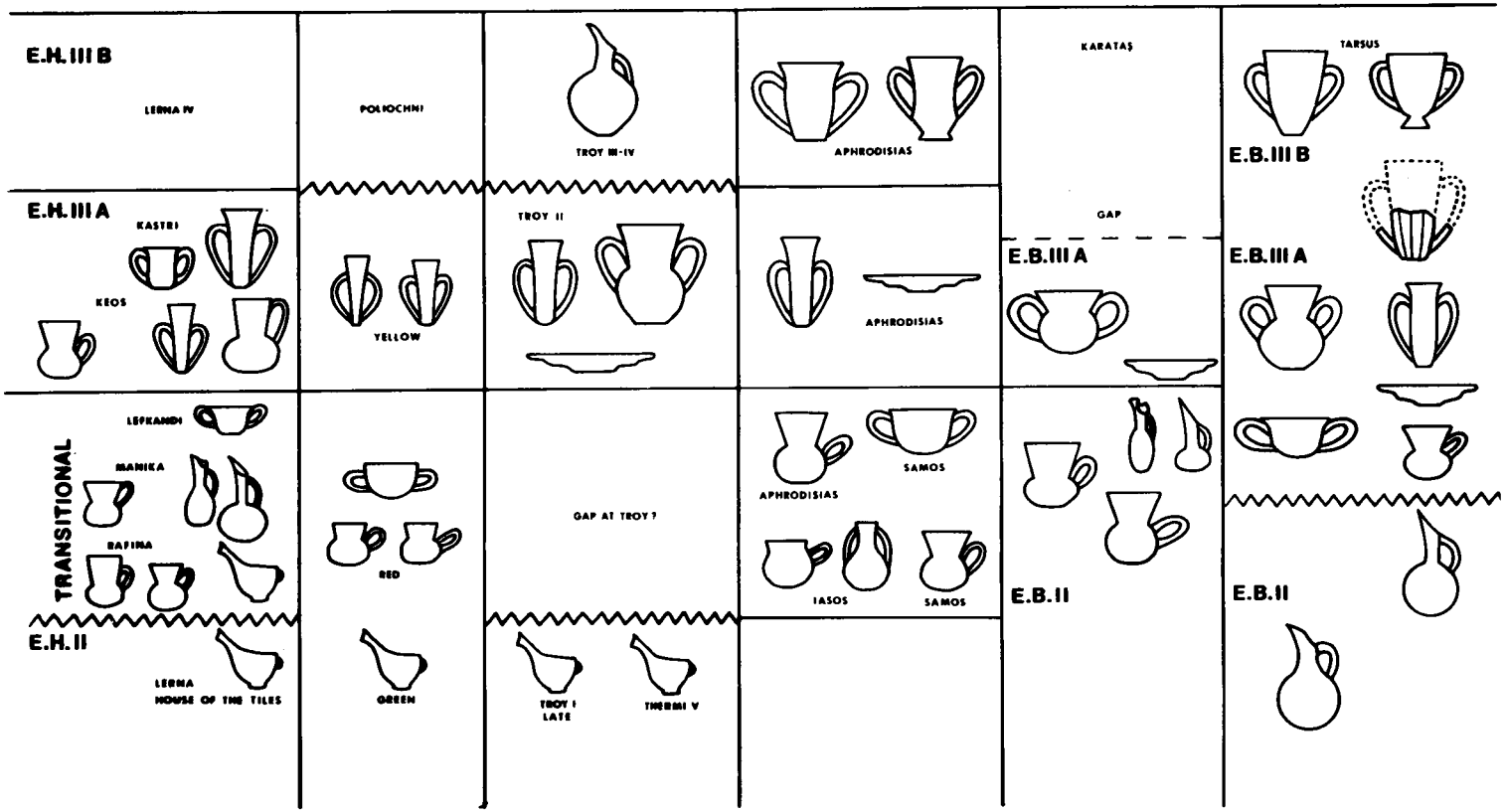

Plate 16

Chronological Chart

tensively excavated for this period. Relatively new evidence comes from Demirci Hüyük, inland in the Mysian-Phrygian zone (Korfmann 1978; 1979), from Beycesultan (Lloyd and Mellaart 1962), Aphrodisias (Kadish 1969; 1971), and Karataş (Mellink 1965; 1968). Precise stratigraphic correlation within the Troy I period is difficult because of the regionalism of the pottery traditions.

The relationship of Troy I vis-à-vis the Aegean should be determined with the aid of the Early Helladic II sauceboat sherds recognized by Blegen in the later subperiod of Troy I (Blegen *et al.* 1950: 186: EH 578; 193: EH 448, 572). As Renfrew points out, imported sauceboat fragments also occur in Poliochni Green-Red (Renfrew 1972: 208; Bernabò-Brea 1964: pl. 129c) and Thermi V (Lamb 1936: 90-91, 130, fig. 32, no. 521). Also found in Troy I (late) was a handle which appears to be of Cilician EB II type, EH 665 (Blegen *et al.* 1950: 170, fig. 250, 7; Mellaart 1957: 83). The late phase of Troy I should (if we trust these indications) overlap with Early Helladic II and Cilician EB II. Troy I was burnt and levelled extensively before the building of level IIa (Dörpfeld 1902: 49; Blegen *et al.* 1950: 82). Although Blegen denies other signs of major discontinuity, the disruption at the end of Troy I may mean a hiatus in the Trojan sequence, in the course of Early Helladic II.

The pottery of Troy I is conservative and does not show innovations toward the end phase. Some of the sites which run parallel with Troy I seem to have signs of a phase intermediate between Troy Ij and IIa. At Poliochni in the Red phase, one-handled tankards with flaring rim make their appearance (Bernabò-Brea 1964: pl. 143). Tankards of this kind (as well as depa and wheelmade wares) seem to be absent from most cemeteries in Mysia, so far as known from the scattered holdings of museums. The suspicion exists that many cemeteries, and with them many EB villages in Mysia, end at or soon after the time of Troy Ij. Among the Yortan material there are a few tomb groups that extend into the early Troy II period (CVA, France, 5: IFb, pl. 2, 30-37; Hutchinson 1935: pl. 29, 3a, b), but these are exceptional. The necropolis at Iasos has just the beginning of the ceramic type of one-handled tankard (Levi 1962: 564, 568, fig. 97, 8). It is strongly represented at Aphrodisias (Kadish 1971: fig. 4), weakly at Beycesultan (Lloyd and Mellaart 1962: fig. P 46, 2). The cemetery and village of Karataş have a ceramic phase at the end of EB II in which one-handled, red-polished tankards with flaring rim, some of them wheelmade, and red-polished lentoid flasks with cutaway rims begin to be used alongside traditional EB II red juglets and beaked pitchers (Mellink 1964: figs. 14, 18, 29: 1965: fig. 12). These groups antedate the introduction of two-handled tankards, wheelmade plates, depa, large red platters, and new storage jars (Mellink 1965: fig. 36; 1967: figs. 43, 46). The initial appearance of the ceramic signs of a 'new era' in West Anatolia is clear at Karataş, in a modest fashion, at the end of

EB II; it precedes the introduction of ceramic forms that are known to us as characteristic of Troy II (which at Troy appear with the introduction of the wheel in IIa-c, not always clearly identifiable in their earliest occurrence).

Troy does not seem to be the place of origin of the one-handled tankard in its typical form with a rounded, rather squat lower body set off clearly from the flaring collar. The handle is a bold loop attached to the shoulder of the lower body (Poliochni, Iasos, Aphrodisias, Karataş); it may also swing up from the lower body to be looped against the surface of the collar (Karataş, Burdur area). The one-handled tankards from Troy II, shape A 39, lack the clear demarcation between body and collar, although some specimens seem to be imitations of the type better known elsewhere (Blegen *et al.* 1950: fig. 378, 33.229 from level IIc, 36.845 from IId, wheelmade). The unstratified examples from Schliemann's excavations are technically late and wheelmade (Schmidt 1902: nos. 1185-97, with offset collar; cf. nos. 1761-65, without offset). Tankards of shape A 39 from Troy III are of incurved profile without the sharp offset (Blegen *et al.* 1951: figs. 68-69). Troy seems to lack a phase in which shape A 39 appears as a precursor of other, two-handled Troy II shapes (but cf. Blegen *et al.* 1950: 64: "not identified with certainty in Troy I, may occur at the end of Late Subperiod in Fine Polished and Luster Wares"). Shape A 42 (Blegen *et al.* 1950: fig. 226, 35.542 from Troy Id) is not a true member of the tankard family.

Is the appearance of these one-handled tankards in Poliochni Red, at Iasos, Karataş, and Aphrodisias (in complex VI, associated with wheelmade plates) a sign of regionalism or a chronological signal of a phase lacking at Troy? Such tankards became known elsewhere in the Aegean and were imitated in the Cyclades (Delos, Syros, Siphnos), on Euboea, in Attica (Rafina) and Boeotia (Orchomenos, Eutresis, Thebes). At Rafina, variants of one-handled tankards were found with sauceboats and other typical EH II wares in house A (Theochares 1952: figs. 10, 12). Those from Orchomenos (Kunze 1934: 55, pl. 22: 2-4) are described as EH II in technique, and Kunze noted the characteristic handle attachment '*wobei das angeklebte Stück oft tief herabreicht*'. Anatolianisms are finding their way into the ceramic repertoire of late EH II in central Greece, and the one-handled tankards are the first to become popular.

To check the chronological correlations, the position of the Euboean cemetery at Manika is to be tested vis-à-vis sites in western Anatolia other than Troy. The Manika tombs contain handmade, one-handled, red-polished tankards of a type developed beyond that of Poliochni and Aphrodisias. The handle is swung up and attached to the rim of the slightly flaring collar (Papavasileiou 1910: pl. H upper row, right), but the squat lower body resembles the South West Anatolian type more

than any piece from Troy, including the wheelmade Schliemann specimens (Schmidt 1902: 1198 or 1761-64).

These Manika tankards (only one of which has its original handle preserved) were found in association with lentoid pitchers with cutaway spouts and sharply beaked globular pitchers, all of these still handmade (Papavasileiou 1910: pl. Θ). The lentoid pitchers, (with their cutaway, flaring spouts of EH II affinity) may resemble spouts found at Troy by Schliemann (Schmidt 1902: nos. 1316, 1360-62), but complete lentoid pitchers found at Troy have relief ribs along the 'carination' as a sign of manufacture in two halves, and leaning necks (Schmidt 1902: nos. 630-36). The simple profile of the Manika lentoids is closer to that of the miniature lentoid from a late EB II Karataş tomb (Mellink 1964: pl. 82, fig. 29), found in association with a one-handled tankard with low handle, a small barbotine jar, and several marble idols, all of this acompanying a multiple burial of a woman aged about forty-two and four children aged six to eleven. The lentoid flask continues at Karataş into EB III in the same non-carinated form (Mellink 1967: pl. 82, fig. 42).

The Manika *Schnabelkannen* have usually been compared to pitchers from Troy IV (Blegen, *et al.* 1951: 9, 110; French 1966; Renfrew 1972: 206). Schliemann's 'long-necked oenochoae' from the fourth city indeed are relatives of the Manika pitchers (Schliemann 1881: 548-54), but there is a difference in body profile. The Trojan pitchers rarely have the squat globular body combined with the long necks and handmade technique of the longbeaked Manika pitchers (Papavasileiou 1910: pl. Θ, lower row, 1 and 2 from left); the wide-spouted Manika type (pl. Θ, top row left, lower row right) is not known in complete examples from Troy (Schmidt 1902: 1284-87; 1311 is a pastiche). It again looks as if the Manika pitchers are not quite Trojan but of some other Anatolian family; their date then is not strictly tied to the suggested Troy III-IV parallels. The Anatolian beaked pitcher is not a Trojan invention, but we do not know its West Anatolian development well. Examples from late EB II tombs at Karataş (Mellink 1965: pl. 61, figs. 13, 14) show a long-necked, squatbodied type in Northern Lycia, but the handle is attached to the rim, not below the rim as in the Manika pieces.

The absence of depa and two-handled tankards from the Manika groups and the Karataş counterparts is perhaps a chronological indication, and the Anatolian component at Manika may tentatively be referred to a West Anatolian complex antedating Troy II. Karataş is but a simple representative of a ceramic and cultural province which in the Elmalı plain itself is represented by many sites, including the mound of Hacımusalar-Choma, of urban size. The source of the Anatolian traits of Manika may be located somewhere between Euboea and northern Lycia, in the southern half of West Anatolia rather than in the Troad.

The second group of Anatolian ceramics on Euboea comes from the settlement of Lefkandi I. Among the published vessels, which are described as mostly, though not all, wheelmade, are one-handled tankards with handles similar to the South West Anatolian type, wheelmade bowls and plates, and fragments of bell-shaped, two-handled cups (Popham and Sackett 1968: fig. 7). Here we add wheelmade wares to the repertory, including the plates so characteristic of Troy II and also of Karataş EB III, and the bell-shaped cup or "Lerna cup" (nicknamed for a gray marble cup from Lerna found 'in and immediately over the burnt debris' of the House of Tiles (Caskey 1956: 164, fig. 4, pl. 47I). We may have moved in time but not in the geographical direction of Troy, where the bell-shaped cup is unknown. Our best specimens of such clay cups come from Tarsus EB IIIa (Goldman 1956: 142, fig. 266, nos. 488-89). The Cilician bell-shaped cup is a wide and deep red-polished vessel provided with sturdy symmetrical loop handles which swing out boldly from base to below the rim, with the characteristic tendency to close the loop upon itself by bending the top of the handle along the upper part of the cup. The bell-shaped cup appears in the earliest strata of EB III Tarsus. The ceramic repertoire of Tarsus, a well-known participant in the new-style ceramics of the Troy II-III era, does not match that of Troy so much as that of a hypothetical common ancestor in the South West Anatolian region, witness also the articulate shapes of the one-handled tankards (Goldman 1956: fig. 266, 467). Karataş, so far, has not yielded examples of the bell-shaped cup, which should chronologically belong in an early phase of the wheelmade plate and tankards series (Spanos 1972: 16-21): A somewhat ungainly bell-shaped cup comes from Poliochni Red (Bernabò-Brea 1964: pl. 147b).

On Keos, at Ayia Irini, in the material published by Professor Caskey as a late phase of the Early Bronze Age (Caskey 1972: 370-75), postdating EH II/EC II, the Anatolian ceramic components include one-handled tankards of traditional South West Anatolian shape (C44) as well as variants (C1, C42, C43) in red and black burnished wares, tall depa in black burnished and unslipped wares, a lentoid flask, and a beginning of wheelmade bowls (C36). The family here includes the depas shape, which seems to mark an advance in time beyond the stages of Manika and Lefkandi, but it still retains the one-handled tankard in shapes unfamiliar at Troy.

At Kastri on Syros, Bossert found depa associated with narrow variants of bell-shaped cups; the accompanying beaked pitchers are of less extravagant forms than those at Manika (Bossert 1967: 53-76; Hood 1971: 435). The group is incompletely published and therefore hard to correlate, but again the variant of the two-handled cup is not of Trojan type.

To sum up: the well known Aegean occurrences of Anatolian Early Bronze Age ceramics have non-Trojan traits. There also seem to be indications that the earlier

groups (Manika, Lefkandi) belong to a stage antedating Troy II, rather than being contemporary with Troy III-IV. The Keos and Kastri finds, with their depa, can be assigned to the early Troy II phase, but they too have non-Trojan elements.

If we consider the chronological indications as valid (against the prevailing trend of interpretations), the Manika-Lefkandi material could belong to a phase missing in the transition from Troy Ij to Troy IIa, but represented elsewhere in western Anatolia. This phase would be correlated with the end of Early Helladic II (cf. the material from Rafina House A) and the beginning of Early Helladic III. The stratification on Keos takes us into the time range of EH III and EC III, and links up with part of the Troy II sequence.

The center of origin and diffussion of the ceramic innovations culminating in Troy II is here attributed to another West Anatolian region. In Caria, Lycia, and adjoining inland territories, red-polished wares, which become so popular in EB III, played a major role in EB I-II ceramics, as opposed to the dark burnished wares of Troy I and the Yortan complex. Contacts with Cilicia provided the impetus to experimentation with the potter's wheel towards the end of EB II, leading to the prolific output of wheelmade plates as household items. The site of Aphrodisias in Caria, which has sizeable stratified deposits of the EB II and III periods, can be expected to provide us with answers concerning the chronology and the nature of the development (Kadish 1971: 121-40; Joukowsky 1978).

What is noticeable in Cilicia is an offshoot—still in ceramic context—of the red-polished tankard-depas fashion. Tarsus shows that one-handled tankards and bell-shaped cups are introduced suddenly, in the phase that can be noticed West to the Aegean as far as Lefkandi I. These two ceramic expansions can be seen as radiating from intermediate Anatolian centers at approximately the same time. At both Lefkandi and Tarsus, we have evidence for more than ceramic activities.

The town of Tarsus had its ups and downs in the EB II period with conflagrations followed by retrenchment and emergency building of fortification walls. The final stage of EB II ended in a violent, third conflagration (Goldman 1956: 31-32), after which a new set of houses was built (the densely hatched walls in Goldman 1956: plan 9). This rebuilding over the burnt ruins of Tarsus EB II is approximately the phase of Lefkandi I ceramics, here with the appearance also of red platters and a fluted black depas fragment of non-Cilician, non-Trojan type (Goldman 1956: fig. 285, no. 722). The new style pottery is found in ample deposits, especially in numerous bothroi, and also on floors of megaroid houses (Goldman 1956: plans 10-12) built in several phases of the EB III period.

Evidently, after the destruction of the EB II town (or at least the excavated part of it) some new architectural forms and customs intruded. These intrusive features

must have been brought in by West Anatolians whose presence can be postulated at Tarsus and probably at several other sites in Cilicia, e.g. Mersin, in the EB III phase. There is no telling who exactly these newcomers were but they must have probed the coasts of South Anatolia as sailors and raiders, and in the case of EB IIIA Tarsus, some settlement resulted. The newcomers must have shared Tarsus with its old inhabitants, and they were exposed to (or actively sought) economic and cultural contacts with northern Syria, as in noticeable in the archaeological record of Cilician EB III and MB I.

Ceramically, the impact of the new EB IIIA features (which develop locally from Troy II to Troy III-IV fashions) spreads even across the Amanus mountains. At Gedikli Hüyük, cremation burials of the EB III period are equipped with numerous imported and locally made depa, wheelmade two-handled tankards, and many lentoid flasks with cutaway spouts, along with local wheelmade wares and Syrian bottles (Alkım and Alkım 1966: fig. 34; Alkım 1968: figs. 21, 23, 40). A depas of footed, Cilician EB III type has turned up in the Amuq (Braidwood and Braidwood 1960: fig. 349) and a round-based, red-burnished depas came from a burial of level III at Tell Selenkahiyeh on the right bank of the Euphrates East of Aleppo (van Loon 1968: 21). The extensive imitation of West Anatolian pottery as far away as Gedikli Hüyük proves the attractiveness of the new style wares to neighbors and outsiders; it also provides chronological, economic and cultural clues.

The Cilician evidence then leads us to assume that centers of enterprise existed in West (South West?) Anatolia and that navigation linked these centers to the coasts of Cilicia by the end of EB II, with some aggressive action at the beginning of EB III. The Trojans, in this interpretation, would not have been responsible for the attacks on Cilicia. We may rather think that Troy Ij fell a victim to rival warriors from the West Anatolian coast, to be rebuilt by a different dynasty after an interval. Other victims of the hypothetical West Anatolian aggressors may have been, at various intervals, Thermi V, Emporio (?), those of the villages in Mysia where 'Yortan' cultures come to an end with or shortly after Troy Ij, and Beycesultan XIII (end of EB II). These attacks are not to be seen as monopolizing efforts by one central power. Undoubtedly riveal leaders waged war among themselves and carried out looting expeditions inland and along the coasts.

There was also exploratory enterprise in the Aegean. The western chain of contacts: Samos, Delos, Syros, Keos, Attica, Euboea, Boeotia (?), Thessaly would suggest trade and some form of settlement here and there (Manika? Lefkandi, Kastri on Syros?). The period in which the Aegean activity of West Anatolians begins is the later part of Early Helladic II on the Greek mainland. The end of Troy I is not far removed in time from the looting and destruction of the House of the Tiles at

Lerna, and from other violent interruptions in the Early Helladic II world. The possibility arises that we could see the end of many EH sites as a result of looting expeditions by West Anatolians, and Troy II as a triumphant station built by a successful warrior king of the new breed of West Anatolian navigators and traders.

These West Anatolian potentates developed their wealth in the Anatolian EB III period and turned increasingly to the East with its channels to the commercial and technical resources of Syria and Mesopotamia. The 'colonization' of Cilician sites is one outcome that can be reconstructed hypothetically. Metal and jewelry hoards were accumulating in the strongholds, rivalling in material, if not in artistic wealth the holdings of central Anatolian kings. Weapons and tools were manufactured at the major stations, and access to tin supplies had been secured (Troy II, Kastri), although we shall leave the sources free from conjecture here.

The relationships with the Aegean seem to have been in the form of some control or peaceful association for part of the EH III period. Troubles of major proportions followed. Troy II, in spite of its powerful fortifications, was captured and looted. Poliochni (Yellow) went under in disaster. The villages and towns of the Elmalı and Karataş area, reduced in number by the regime of the EB III rulers, dwindle and the survivors move to sites on higher ground, to become visible again in the Middle Bronze Age under the aegis of proto-Hittite and Old Hittite culture from the central plateau.

The end of the EB III period in West Anatolia leaves few citadels and settlements intact and prosperous. Did the rulers end their own progress by internecine warfare, or did a major outside force put an end to their wealth, their ships, and their organization? Three external powers are to be taken into account.

The Minoans and their Cycladic allies remain an important factor. Least known of all EBA communities, the Minoans emerge as the wealthiest and most ambitious Aegean kings, traders and sailors at the beginning of MM I. We shall have to ask our Minoan expert about this aspect of the unwritten history of the end of the Early Bronze Age.

A second external force that could have interfered with the West Anatolians is that of the kings on the Anatolian plateau, who knew their western neighbors through trade routes and exchanges. On the plateau there were also troubles, wars, destruction levels in the EB III period. We note that large old towns like Kanesh-Kültepe had a period of great prosperity preceding the documented trade contacts with Mesopotamia of the twentieth century BC. In the course of EB III, the lower town of Kanesh was expanded to enormous size (Karum IV-III), a phase for which we do not yet have tablets. Many people must have flocked to the old centers, voluntarily or driven by force and danger, in the warlike era when in the West sites like Troy II went under.

Finally, it is possible that the major threat and action came from Mesopotamia itself in the form of campaigns on the Anatolian plateau and in Cilicia by Sargon and Naramsin of Akkad. Their activities were well organized and resulted in punitive campaigns and the establishment of strong control stations in conquered territory. Did Akkadian armies indeed venture as far as the Anatolian plateau, following reports of their merchants who knew the territory, and carry out campaigns of looting and destruction? This may be true for the Eastern confines of the plateau (e.g. Norşuntepe), even for towns on the plateau itself (Alaca Hüyük suffered a major destruction), but we should hardly expect a victorious Akkadian expedition to have pushed its way into West Anatolia and to the gates of Troy II. Yet, the West Anatolians were participants in an era and in contacts which extended from the Aegean to the literate and energetic economic regime of Akkad. If they styled themselves as conqueror kings, they paid the price of their ambitions in defeat and in a loss of privileges and stimuli which came with wealth and trade.

Troy III-IV went into a decline, and the increasing activities of Troy V point to closer interaction with the Anatolian plateau. This is true for South West Anatolia as well. The initiative in Anatolia passes to the Indo-European speakers who had been establishing themselves among the central Anatolians and who will found the Old Hittite dynasty. Their first major exploits were raids against North Syria and Babylon at the time of the Shaft Grave dynasty in Mycenae. West Anatolia tried to maintain its independence, but the initiative in Aegean seafaring had passed by 2000 BC to the Minoans, who soon began to gain footholds on West Anatolian shores.

WHY WAS CRETE DIFFERENT?

GERALD CADOGAN

(Plate 17)

In 1960 Jack Caskey re-defined the change from the Early Bronze Age to the Middle Bronze Age in southern Greece. That is where we have begun in this Symposium, as everybody else has begun since he wrote 'The Early Bronze Age in the Argolid' (1960). In Crete things were different, but not so different as is often thought, as Professor Caskey pointed out in 'Crises in the Minoan-Mycenaean World' (1969). The destructions of Early Minoan II needed, and still need, explaining; but they were not followed by anything like the recession we discern in Middle Helladic Greece and in contemporary western Anatolia. Instead, over the next few centuries the political, economic, artistic and religious centres appeared that we know as palaces. The palaces of Crete are a symptom, and were a cause, of great changes. Something had to start them. Once started, they were the basis of Minoan life for half a millennium, until brought low probably by the Mycenaean successors of the Middle Helladic recession.

In this paper I shall try to examine what was different in Crete around 2000 BC, and why it was different, and how different it really was.[1] What caused the Early Minoan II destructions? How much disruption did they bring? What was the state of society? How did the idea of palaces come about? A Cretan invention alone? Perhaps an idea developed from such buildings as the House of the Tiles? Or somehow obtained from the Near East? Why did the palaces prosper? Why does Crete seem so self-contained in the Old Palace period?

The task is difficult, as we have so little evidence for Crete in the Early Bronze Age. With the Old Palace period we are better off but still far from the precision we expect when studying the New Palace period. Most of the evidence for the development of culture comes from communal tombs, whether rock shelters or built house-like tombs or tholos tombs, many of which were used for hundreds of years and contain many burials. Few have sealed deposits. Yet the tombs contain almost

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[1] I am most grateful for comments made at and after the symposium, and at a meeting of the Mycenaean Seminar in London in October 1980, when an altered version of the paper was given (summary: Cadogan 1981), and from Professor Keith Branigan. Dr. Stylianos Andreou has helped greatly. Responsibility for the paper stays, as usual, with the author.

all the Prepalatial seals, stoneware and metal objects we know, and many of the Protopalatial objects and imports from abroad. All perforce are difficult to date; which in turn makes it very difficult to estimate the progress of technology or the spread of wealth. The few settlement levels, principally at Debla, Fournou Korifi, Gournia, Khamaizi, Knossos, Mallia, Palaikastro, Phaistos, Pyrgos, Tylissos, Vasiliki and Zakro, are invaluable but hardly enough.[2] For a start, we still need far more evidence to be sure of the relative chronology of the Prepalatial and Old Palace periods and have a framework for events. One group of potential problems, however, is not a difficulty: it is generally agreed now that the same people were in Crete in Old Palace times as in Prepalatial times, and that no immigrants came after Early Minoan I (Warren 1973), if any then. The archaeological record does not show any, except possibly the hint of a few people from the Cyclades at Ayia Fotia, in a context which would anyway seem to be mainly Early Minoan I (Davaras 1971, 1976: 128-29; Doumas 1976, 1979). The sceptic may think of instances where archaeology does not reveal, or barely reveals, immigrants.

Early Minoan I is especially sketchy, with its problems of possible immigrants and of a possible Final Neolithic, or a "Sub-Neolithic", to account for the continuum in Crete (Vagnetti and Belli 1978; J. D. Evans 1971: 113-14; Renfrew 1964; 1972: 71-72; Vagnetti 1973: 118-23; Warren 1973). We have no need to discuss the details here, luckily. There was at least a fundamental continuity of culture from Late Neolithic to Early Minoan, among the peasants, a point that Sandars has emphasized recently in a different context (Sandars 1978: 24). Continuing pottery traits, such as scribble burnish, the chalice shape and wiped surfaces, support this (Evans 1928: 10-12, figs. 3m, 4; J. D. Evans 1964: 225-29; Hood 1966; Hood and Cadogan forthcoming; Warren 1973: 42). During this phase the population grew, more rapidly than before, with several new settlements in North- and South-central Crete, and also in the East and the West (Warren 1973: 41; Warren and Tzedakhis 1974: 338-39). The first communal tholos tombs were built. The founding of settlements by the sea and/or on high and defensible hills (Alexiou 1979; Hood, Warren and Cadogan 1964: 51) may suggest newcomers, as may some pottery traits that appeared. Pottery burnished with patterns on a reserved surface can be linked to that of the West coast of Anatolia, and so can some shapes of vases found in tombs. Any movement may have begun already in Late Neolithic (Warren 1973: 41-43). Equally, to explain the appearance of dark on buff painted ware, we tend to look towards Syria or Palestine, or perhaps South Anatolia; and other Syro-Palestinian parallels

[2] Warren 1980 is an excellent recent survey of the evidence, with refs. See also Andreou 1978 and Wilson 1979.

have been adduced (Warren 1973: 42, with refs.): but the evidence is faint. The only imports in Crete are Early Cycladic I bottles of the Grotta-Pelos or Pelos-Lakkoudhes type from the Pyrgos burial Cave (Xanthoudides 1918: figs. 8: 44-45, 9: 67-69), which maybe arrived in EM I or maybe later, and from Ayia Fotia (Davaras 1971: 303, pl. 391β); and Melian obsidian. As evidence for immigrants the pottery traits are matched by the two leading theories in deciphering Linear A—if the script was used for just one language.

Early Minoan I was a long phase, four or five hundred years or more. The growth of population, that is seen often as an especially important feature of Early Minoan II, may already have begun in late Early Minoan I, if we may take light grey burnished ware (grey throughout) and grey burnished ware (usually with a reddish core, like Argive Minyan) as being as much a sign of an EM IB as of EM IIA (*pace* e.g. Warren 1972 (2): 396). The results of Mr. Hood's excavations beside the Royal Road at Knossos may support the idea, and the first occurrence of the light grey ware is from what we may call EM IA, in a well inside the later palace at Knossos.[3]

If we concede this, the growth of population will have to be more gradual. Still, there is good evidence of growth around the turn of Early Minoan I to Early Minoan II, with many new settlements (Warren 1972 (1): 272; Warren and Tzedakhis 1974: 339-40) and plenty of evidence of new technonology and a real prosperity. It has been suggested that this was the time when the olive and the vine were domesticated (Renfrew 1972: 281-87): in Crete, at any rate, tripod cooking pots appeared, joining baking plates known from Early Minoan I (Hood and Cadogan forthcoming; Warren 1972 (1): 125), and people perhaps had better meals. With Early Minoan II the Mesara must have been quite densely inhabited, and it may be that people came over from the South and less friendly side of the Asterousi mountains (cf. Blackman and Branigan 1972: 67-68). The many communal tombs show the need for organisation, which may be implied also by the many seals found in them, a new part of life in Early Minoan II. The seals and sealings of Fournou Korifi show that seals were used for sealing as well as being amulets (Warren 1970; 1972 (1): 226-27). The basis of the wealth of the Mesara may have been copper (cf. Branigan 1971: 14); many of Crete's scarce resources are there, perhaps quickly exhausted, if ever used at all (cf. Wheeler *et al.* 1975). It is likely that it was agriculture that was the main support of the settlements round the plain.

The settlements excavated are far too few to give us a clear idea of Early Minoan society. Fournou Korifi is a small close-knit village, with a suggested population of 100-120 (Warren 1972 (1): 267) or 20-25 (Whitelaw 1979). It seems to have grown

[3] Further discussion in Hood and Cadogan forthcoming.

from the inside out, and been quite self-involved and fairly well organised, if very irregular in its adapting to the terrain. There were probably several separate houses, as Whitelaw and Wilson have shown recently, and separately (Whitelaw 1979; Wilson 1979). Somebody must have been in charge, but there is nowhere we may place him, her or them. As with so many Early Minoan settlements, defence was a factor (Alexiou 1979): Fournou Korifi (Pl. 14) is on a dry hilltop, with traces of a perimeter wall, and perhaps a bastion ('Room 62': Warren 1972 (1): 56-57) to protect the main entrance. Its water source would have been down below in the valley. Many other Early Minoan settlements are in similar spots to Fournou Korifi, unprepossessing and with few natural advantages save a fine view. Vasiliki is somewhat different. It used to have the 'House on the Hill' (Seager 1905, 1907). That has now become two houses beside a courtyard (Zois 1976), good houses with a complicated history, larger rooms than at Fournou Korifi and a strong sense of better construction and greater prosperity. The impression may be false; but the land is good for farming around Vasiliki today, and probably was so then. One of the two houses is at the corner of the court. They remind one of the New Palace period houses at Tylissos, or perhaps even look forward to the arrangements of the palaces themselves. Other fairly substantial buildings are at Palaikastro (δ 32, χ 32: Dawkins 1904: 199-200, 1905: 272-74) and at Phaistos, where there seem to have been smaller houses around (cf. Warren 1980: 489, n. 45, with refs.) a clue perhaps to class.

The same probably holds for Knossos, where J. D. Evans has excavated the basements of a prosperous house below the West Court, with some fine pottery including a splendid bowl that could well be called palatial (J. D. Evans 1972: 122, 124 fig. 11): it is perhaps the first true krater of the Aegean, if that is wine was introduced in Early Bronze II. (In terms of function, then, it would complement the so-called sauceboats of the Cyclades and the Mainland.) The other Early Minoan II buildings appear humbler, in so far as they have been dug. By the South front of the palace they are terraced into the hill (Hood and Cadogan forthcoming) as the West Court house presumably was: which might suggest some ruling house at the top of the terraces somewhere by the later Central Court or, incorporating J. D. Evans's house, where the West wing of the palace is. The building by the Royal Road excavated by Warren has been claimed to have an obsidian workshop (Warren 1972 (2), but see Torrence 1979: 77-79). All the Early Minoan II buildings known at Knossos have plenty of fine pottery, including the elegant EM II version of Ayios Onoufrios ware, well known for instance at Koumasa (Xanthoudides 1924: pl. 26b).

The distribution of this pottery (cf. Zois 1967) might suggest a regional grouping that covered North- and South-central Crete; but equally the ware is the finest of

Early Minoan Crete and may well turn out to have been made all in one place and traded. Clay analysis may help. The light grey and grey burnished wares might also suggest a loose grouping, as does the general picture of prosperity, and chieftains or rulers and organisazation, that we may put together in Knossos, Arkhanes and central South Crete. There were of course grey wares also in the East (if derived perhaps from the centre), followed by links with the centre in such things as Vasiliki ware (Betancourt 1979: 27-28). The use of rectangular house-like tombs rather than circular ones was particularly favoured in all East Crete. Krasi and Myrsini may be seen as outliers of the zone of tholos tombs, and Arkhanes as a place where the two ways of burial met. As for wealth, that of Mochlos matches any in the Early Bronze II Aegean—but is not up to that of Troy of what Miss Mellink has shown us should be Early Bronze III, in Aegean terms. In West Crete I suspect that if more could be dug around Khania, we should find wealth there also. (We shall never know how early wealth appeared at Khrysolakkos at Mallia. The first phase ascertainable of the building, with dressed masonry, is Early Minoan III or Middle Minoan IA (Shaw 1973 (1)).)

We are beginning to have a picture of local peculiarities, some shared, and centres of wealth in the different regions of Crete, although in all the culture is indisputably Early Minoan II. Of course, there is much to be published and correlated; but we may find a clue here to explaining the various destructions of Early Minoan IIA and Early Minoan IIB, even a possible tilt of Knossos to the East, and something to presage the later evidence for regional groupings and centres.

Crete shared in the new technology of the Aegean in ways of adorning oneself and in metal daggers and weapons. The resemblances may be no more than general, similar reactions to, and similarly part of, a general enrichment. The seals and stone vases are similar general evidence, although with the stone vases one might make a good case for a Cycladic influence on, or spur to, their development in Crete.

It is difficult to imagine how hard the international spirit blew in Crete. With the Cyclades (Renfrew 1972: 199-200) there was a parellelism in some of the stoneware, metalware and jewellery, and in painted pottery; the Cyclades were the source of folded-arm figurines which were perhaps imitated in Crete; the source of obsidian (although a little started to come from Anatolia in Early Minoan II ((Hood and Cadogan forthcoming)); and the possible source of lead and silver—this was the time of the first silver cups in Crete (Davaras 1975: 102, 107, pl. 17; Seager 1912: 52, fig. 22, VI. 8); and defence, war and wealth. It was not a cultural *meltemi*. The Cyclades's links were far closer with Helladic Greece, which is something confirmed by what happened in the succeeding phase. The push then went West through the Cyclades to Euboea and the Greek mainland (Mellink, *supra*). The route did not lead

to a (by then) Early Minoan III/Middle Minoan IA Crete. The adoption of spirals, however, in East Crete in Early Minoan III on pottery, with their probable origin in the Cyclades, would argue against this, *if* the spirals came immediately in Early Minoan III; but the inspiration may have come as easily from Early Cycladic and Early Minoan II stoneware, as it could similarly for the decoration of hatched triangles in East Cretan Early Minoan III. (A few isolated spirals are, of course, known on Early Minoan I pottery (Hazzidakis 1913: 38-40, fig. 3; Hood and Cadogan forthcoming), and plenty of hatched triangles, continuing into Early Minoan II, whose ultimate source was probably woodcarving.)

As evidence for links with Greece, a few sauceboats have been found in an Early Minoan IIA context at Knossos (Warren 1972 (2)), and at Platyvola in West Crete (Tzedhakis 1965, 1966, 1967, 1968) and a likely Minoan seal at Ayios Kosmas in Attica (Mylonas 1959: 29, fig. 166, 13). On Kythera the small Early Helladic settlement of Kastraki was abandoned presumably when nearby Kastri, 100 m. away, was settled from West Crete, the earliest pottery being Early Minoan II (Coldstream 1973; Coldstream and Huxley 1972: 83-91, 275-77). We may speculate how the change happened. It is difficult to imagine it as being peaceful. The foundation of Kastri is another important mark of the growth of Early Minoan II: it too may have had some share of the wealth if such things as the small stone vase with the cartouche of User-Kaf of the Vth Dynasty (Coldstream and Huxley 1972: 266, fig. 95, pl. 86, P1)—a chance find — arrived in Kythera soon after it was made. A big if. Kastri has good farming and excellent spots to draw ships up.

Cretan links with the Levant are even more vestigial: perhaps an Egyptian stone vase or two (Warren 1981: 632-35) and some ivory (if it is really from elephants), as imports. To keep the links vestigial we must allow that the lions and monkeys of the seals were then extant in Crete. Faience appeared in Early Minoan II, a bowl and some beads, perhaps local, or more probably from somewhere such as North Syria, whence the Minoans may have also learnt how to make faience (Foster 1979: 56-59) and, it seems, there was an Akkadian silver cylinder seal (Kenna 1968). Both the faience and the seal come from Mochlos.

The evidence of Early Minoan IIB destructions is not enough to suggest trouble throughout the island as in Late Minoan IB, or even at the end of the Old Palace period; but there was certainly dislocation, and also plenty of evidence of continuity of culture and quick resettlement, in some places at least. We must now consider Early Minoan III and its part-contemporary Middle Minoan IA, in more detail than we have given to the earlier phases, as we have here the period immediately before the foundation of the palaces, if not actually that of the foundation of the palaces. It is here that we may find the answers to our questions. The evidence is full of dif-

ficulties, inadequately published, presented only in preliminary reports, dug a long time ago.

We shall go from West to East. In Khania the excavator talks of a "decline in Early Minoan III" (Tzedakhis 1965: 569): we should really like to know far more of that site, as with all its periods. In Knossos a pure pre-polychrome and post-Vasiliki-ware deposit, with one worn example of the typical EM II bowl with inturned rim, was found to the South of the palace, in Mr. Hood's excavations (Hood 1966; Hood and Cadogan forthcoming). Other deposits around Knossos may be linked to it, including that which dates an open cobbled area below a Middle Minoan I road at the far end of the Royal Road (Catling 1972: 21), perhaps to be seen as part of a general rebuilding around Knossos. Another deposit is the fill behind a wall below, and on the same alignment as, the later palace wall in the North West part of the palace, built with long thin isodomic stones. Mr. Hood suggests that it means an Early Minoan III palace, at least here, and more or less of the scale and alignment of the Knossos palaces we know (Catling 1974: 34). Wilson would like to associate it with the building of the Keep, whose special importance is that it defines the North end of the Central Court and the West side of the North Entrance Passage (Wilson 1979). Evans placed the foundation of the palace of Knossos, and of that of Phaistos, in Middle Minoan IB (Evans 1921: 144-46): his evidence at Knossos was chiefly from the South West part of the palace, and hardly more than Mr. Hood's. One building he did ascribe to Early Minoan III is the great "hypogaeum" below the South Porch, something none of us has seen: it was filled with debris, including MM IA (or EM III?) eggcups, to support the Old Palace when that was put up, says Evans. We do not know its use, nor precisely when it was built, but Evans saw it as part of "some Early Minoan predecessor of the great Palace" (Evans 1921: 103-107, fig. 74). He suggested that there was another by the South East corner of the palace.

The Early Minoan III deposit on the South front at Knossos was above Early Minoan II levels. One habit that continued, or rather grew, was the use of white paint on a dark ground, which was quite popular at Knossos in Early Minoan II, more so than at Fournou Korifi. The evidence of both places is important in reminding us that white-on-dark was not something new in Early Minoan III. There is a little evidence for it also in Early Minoan I at Lebena (Alexiou 1960). Early Minoan III at Knossos was a fairly short phase, succeeded by Middle Minoan IA, at the end of which the Old Palace was built, perhaps succeeding a "proto-Palace". The extent of the MM IA settlement is surprising, from quite high up the Akropolis hill c. 500 m. away from the palace, to North West of the Villa Ariadne and down to Makryteikhos (Catling 1977: 22; Evans 1928: 546-57; Hood and Smyth 1981: 8,

with refs.); and, Mr. Hood reminds me, it was about this time that the viaduct and roadway, with the stepped portico, to the South of the palace were built (cf. Hood and Smyth 1981: 8).

In the Mesara some Early Minoan III and Middle Minoan IA may be recognised in the pottery of the tholos tombs, and Middle Minoan IA at Patrikies (Bonacasa 1968). We cannot tell if there was any dislocation at the end of Early Minoan II in the Mesara: the only sign might be that by about Middle Minoan I tombs on the South side of the Asterousi mountains had gone out of use (Blackman and Branigan 1977: 68).

In Mallia there is a little evidence of Early Minoan III, in one spot by the Hypostyle Crypt more or less above Early Minoan II, and of East Cretan (and not Knossian) character (Amouretti 1970: 51-54, 76). This was probably followed by a Middle Minoan IA style running into Middle Minoan IB, as the potter's wheel was used, of which the best deposit is from three houses to the South of the palace (Chapouthier and Demargne 1962; Andreou 1978). These houses are separated by narrow streets and remind one of Early Minoan II Fournou Korifi and Vasiliki. It was in the succeeding phase at Mallia, that is later than the beginning of Middle Minoan IB at Knossos and Phaistos, that the Agora and buildings around, Quartier Mu and the probable Old Palace were begun. We shall come back to them.

In the East Early Minoan III is represented by deposits of cream-on-dark pottery, best known from the North Trench at Gournia (Hawes *et al.* 1908: 57). Fournou Korifi was deserted, for good,[4] and so was Pyrgos, for the phase: the evidence is that it does not have, apparently, any pure deposits of cream-on-dark pottery. When Pyrgos was re-settled, a large communal tomb was built, with a courtyard outside it, and a road and stairs leading to the courtyard. This surprising monument was built on or into a terrace made from the Early Minoan II destruction debris. Its elements, except perhaps the road and stairs, are traditional, and the plan of, and the way of building, the tomb chamber are similar to those of the largest room (no. 80) at Fournou Korifi. Thereafter, the tomb was used throughout the Minoan life of the settlement, though probably intermittently. The first break may have come in the same period, Pyrgos II, as the road and courtyard were covered with gravel yards where people cooked (The pottery is still generally the same, but some vases were made on the wheel, indicating Middle Minoan IB, as at Mallia.) (Cadogan 1978: 71-74). I wonder whether this history could not be seen as a parallel to that

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[4] The arc-shaped building (no. 97) at the top of the hill at Fournou Korifi (Warren 1972 (1): 91-92, fig. 31) is probably a modern beacon-tower and/or watch-tower, like that at Pyrgos nearby (Cadogan 1978: 82-83, fig. 37), and not a building of EM III.

of the tumulus of Lerna (Caskey 1960: 293-94)—both monuments to honour the dead, or the destroyed settlement; both ignored fairly soon after being built—at Lerna apsidal houses soon cut the tumulus—for a better use of the space?

The rest of East Crete likewise took up Middle Minoan polychrome styles, with the cream-on-dark continuing beside them. Then came a phase that corresponds to that of Quartier Mu at Mallia. One immediate problem with it, which we cannot settle now, is whether there was an Old Palace of Zakro, or whether we should be talking of a first phase of the New Palace (cf. Andreou 1978, and Hiller 1977: 140-41, both with refs.).

We may turn to the other evidence. The same habits of burial appear to have continued with Early Minoan III and Middle Minoan IA, except that pithoi and larnakes (the latter the size and shape of wicker baskets, and perhaps intended to copy them) began to be used. They are found both in communal chambers in the Mesara and at Arkhanes (T. E, upper level: Sakellarakis 1975 (1): 269-72, fig. 2) and later at Knossos, and in open cemeteries in central and East Crete.

A more important development was the appearance of mountain shrines. They were established from the end of the Prepalatial period till the New Palace period, and some were in use for long after that. Some are caves, some are buildings; and at least one is a building associated with a cave (Karetsou 1981). I see no important differences in what the various forms would have meant. They imply some organisation, in that one village or group of priests was presumably in charge of each shrine, and worshippers came presumably from several settlements. Some shrines must have had a regional importance; and many shrines are visible from, and appear connected to, the local Minoan centres, towns, country houses and villages, or palaces. This need not mean that their organisation foreshadows that of the palaces. It is far more likely that the shrines were organised informally, haphazardly; and their festivals may have been as infrequent and governed by the seasons as those in Greek mountain shrines today. The offerings in the shrines, mostly small figurines of women and animals, including sheep and dung-beetles, point to the pastoral life of the mountains (Rutkowski 1972: 175-79). Later, Linear B tells us of the importance of sheep, and therefore of their mountain pastures, in the economy of Knossos (Killen 1964): which was presumably the case for Knossos and the other palaces back to Old Palace times. I do not see any direct causal link between the appearance of the shrines and the appearance of the palaces. At best, both may be seen as symptoms of the power of religion in Minoan life and of a desire to arrange, or define, that power in tandem with political power. Even so, they are very different phenomena.

Technology may have advanced quite quickly in Early Minoan/Middle Minoan IA, but here again it is difficult to be sure as so much of the evidence is from communal tombs. Potters started to use the wheel generally from Middle Minoan IB. By adopting the wheel the potters made more fluid and/or crisper shapes, and probably produced more vases more efficiently, perhaps creating an increased demand: the amount of Middle Minoan pottery is impressive. The wheel appeared rather later than in mainland Greece (Howell 1973: 76, 79, 84-86).

In metalware, granulation and filigree came by, and probably in, the time of the Old Palaces, and the same probably holds for the use of the tubular drill on sealstones, which were beginning to be of harder stones, and for stone vases. There is no certain evidence of Prepalatial use, but the evidence accumulates that the foundation of the palaces was accompanied by a fast developing technology, which may have already begun to move in late Prepalatial times. Some, or many, ideas may have been learnt from the Near East, as Branigan has suggested for this period, Foster for Early Minoan II, and Sandars for that of the Old Palaces (Branigan 1968; Foster 1979: 56-59; Sandars 1961: 19-22).

We should also mention ashlar masonry, and health. Ashlar masonry appeared in Middle Minoan I or just before (Shaw 1973 (1)). Its invention, which suggests better bronze tools, must have been a great stimulus to the mastermasons who had to make the palaces or such buildings as the Khrysolakkos cemetry. They also used, besides the dressed masonry, large and more roughly shaped blocks as a significant feature of their building.

As for health, the samples are still small, but are most interesting. In Early Minoan Crete there was greater longevity than in Early Helladic Attica. In Middle Minoan times at Knossos, people were living to a much greater age, and particularly the more vulnerable party, women. Modest improvements in health can lead to a sharp rise in the birth rate and in the survival of children and of the population as a whole (Halstead 1977). There must have been a very fast growth in Middle Minoan Crete, and the general level of health was much better than that of Lerna (cf. Angel 1971). One reason may be that the Minoans could eat more meat, to judge from the preliminary results of the study of the animal bones from Pyrgos, which show a quite high proportion of pork, an animal raised primarily for its meat (Gamble 1979). These results will probably turn out to hold for the Middle Minoan and Late Minoan I levels, and should be contrasted with the low amount of pork bones at nearby Fournou Korifi (Jarman, in Warren 1972 (1): 318-20).

The improvements in health are even more difficult to date than the improvements in technology, but both probably advanced during the Old Palace period. By the time of the end of the period we may instance the many settlements;

in technology, the revival of metal plate, in the silver crinkly-rimmed kantharos from Gournia (Davis 1977, 1979, with refs.) and the bronze cauldrons from Mallia (Vandenabeele, in Detournay *et al.* 1980: 72-89) and, I think, in silver and bronze hemispherical cups from Byblos (Montet 1929: 191-92, pls. 111, no. 748, 113, no. 749; Virolleaud 1922: 279, fig. 2 *bis*); the long swords of Mallia with their elaborate chasing (Sandars 1961)—I trust they may be allowed to be of around the end of the Old Palaces; multi-coloured faience (Cadogan 1976 (1); Foster 1979: 153-55); the production of black-glaze Kamares ware (Noll *et al.* 1971), often of eggshell thinness or with elaborate relief decoration; the use of hard stones for seals (Boardman 1970: 31) and Yiali obsidian for vases (cf. Warren 1969: 136) and buildings three stories high at Phaistos, with levelling courses and window reveals. A remarkable technical inventiveness and a willingness to accept new ideas. We may add the use of scripts, mainly in the palace and their towns; distant and perhaps quite intense foreign trade; the use of pot marks and, at Mallia and Pyrgos, of seal impressions on pots, made before the pots were painted or fired (Olivier, in Poursat *et al.* 1978: 91-97, 106-204; Cadogan 1978: 76); and an agriculture that must have been able to support so much activity.

Foreign contacts may be traced a little more easily in the late Prepalatial period—to return to that—than the other classes of evidence, in that some of the evidence for contacts (which is in general exiguous) is either stylistically definable and dateable Minoan pottery found abroad or foreign objects found in more or less certain Minoan contexts. The pottery, a handful of vases comes from Lapithos in Cyprus, Lerna V and Phylakopi (Catling and Karageorghis 1960: 110-11, fig. 2; Zerner 1978; Cadogan 1979: 64); and at Lerna and Pefkakia there are versions of the Minoan polychrome which had perhaps actually been made in the Argolid (Zerner 1978). More work is needed on what is a most interesting feature of Middle Helladic, the best evidence yet for links with the Minoans in the centuries before the Shaft Graves. The certain imports in Crete are scarab seals, two of which come from later Prepalatial contexts (Ward 1971: 92-94, fig. 13, 1-2; Cadogan 1978) and a Minyan bowl suitable for Lerna Va (Zerner 1978, 1980: 538) from Middle Minoan IA Knossos (Hood 1971), and some Anatolian obsidian (Hood and Cadogan forthcoming). There are also several exports and imports, such as Minoan stone vases in Mainland Greece, the Aegean and the Levant (Warren 1969: 184-85) or the suggested Byblite daggers in Crete (and possibly Cyprus) (Branigan 1966, 1967) and Minoan daggers and other metal objects in Cyprus and Mainland Greece (Catling and Karageorghis 1960) that may belong to late Prepalatial times or to the Old Palace period. It is not surprising then that the story of the growth of Old Palace foreign relations is written mostly in terms of pottery, which may well hide what the

real issues were. Cypriot copper for instance may have been an attraction to the Cretans from as early as Early Minoan III/Middle Minoan IA, or even a necessity (Cadogan 1979: 64-65). Tin was becoming another. As for the South Aegean, nothing suggests any Minoan mastery. Occasional and slowly intensifying contacts, yes. But nothing more. Crete seems to have been, as it were, the sleeping giant on the South border of the Aegean sea, in no way exercising political control on the prosperous island states or on the depressed Mainland. There may have been some, of course: raids, perhaps, and whatever history lies behind the Minoanizing dark lustrous wares of Lerna and elsewhere.

We are left with our original questions. How did the palaces come about? Why did they prosper? Why was Crete self-contained in the Old Palace period, as against having close contacts with, and a very strong influence on, the South Aegean in the New Palace period? I shall try to answer these questions.

The evidence suggests that, despite some abandonments and dislocation after Early Minoan II, there was in general a continuity or habit in ways of living from Early Minoan II to Early Minoan III. Architecture, habits of treating the dead, the types of metalware and stoneware, the white paint on pottery and (perhaps) the use of the same sites are evidence against an intrusive culture in Early Minoan III. Nothing points to newcomers: which does not exclude that there were some. We have to explain how the palaces came about within a way of life which did not experience major changes, and without the convenience of immigrants. Why were they settled where they were? How was the plan around a central court decided upon? What were they used for? How did the idea happen? Who was in charge? Was there any foreign inspiration?

The places chosen for the palaces were becoming or probably were already regional centres, as the evidence of the pottery groups and the evidence of Early Minoan II occupation tend to suggest (Andreou 1978). No palace was founded at a new site. Phaistos on its acropolis overlooks the Mesara, which was steadily gaining importance in so far as the South side of the Asterousi mountains was losing it; Knossos had the benefit of great antiquity—something of value in a conservative island—and the central position in the economy of the island, foreign links and grand buildings in Early Minoan II and Early Minoan III; with the Old Palaces Mallia could have been the capital of a group that embraced Pyrgos and Gournia, and went presumably to the isthmus of Ierapetra; Zakro and its fine harbour we know less about, but we can see that its pottery may form a group with that of the rest of East Crete; and of Khania we know even less, but a similar judgement would probably be right. The site there is on an acropolis, above a good harbour. A pattern then probably of regional centres, none obviously dominant.

Each centre would have been supported by its hinterland, which is best defined in terms of its pottery group and habits of burial, but with time we may be able to add metalware, seals and stoneware as criteria. The hinterlands included prosperous villages, such as those at Vasiliki, Tylissos and Pyrgos or those to be inferred from the Mesara tombs. The villages may have included local grand ruling houses, country houses, such as we know from the New Palace period.

The best preserved Old Palace is Phaistos. As far as we can see, in its arrangement around a Central Court, with main storerooms and religious rooms on the West, and its grand West facade, it was not essentially different from the New Palace. The paved West Court was covered over in New Palace times to the depth of a metre, but there was still a courtyard there. At Knossos, in so far as we may judge from the scanty Old Palace remains, the Old Palace was probably like that of the New Palace. We have no way of telling whether the suggested growth out of *insulae* (Evans 1921: 139-41, 203, fig. 152) really happened. At both places, the *kouloures* in the West Courts were used only in the Old Palace period, those at Knossos being built over Early Minoan III and Middle Minoan IA houses; at Phaistos the building techniques of the Old Palace are remarkably similar to those of the New Palace, and therefore already advanced.

At Mallia likewise there seems to have been little development from Old to New Palace times in building. Mallia and Zakro are difficult sites. We scarcely know if we should be talking of an Old Palace or of a first phase of a New Palace—the recurrent problem of Middle Minoan III—as we have such a vague idea of the buildings below the present New Palace buildings. The "palace" (if that is how we may call the predecessor) at Zakro was not freestanding: buildings were tacked onto it at the North West (cf. Hiller 1977: 139-46, with refs.)—or was it the other way round, that the palace was tacked onto the buildings? It may all just reflect the pressures of little space on the slope of the hill or may tell us something more significant about the relationship of the inhabitants of the palace and their neighbours.

At Mallia we probably do have an Old Palace below the New Palace, to judge from the clues in the West Court and on the West and North sides (Cadogan 1976: 111); but we also have an "Agora" and Quartier Mu to be related to the (hypothesized) palace. 'Agora' is probably the best term (van Effenterre and van Effenterre 1969) for the rectangular court (Pl. 17a), c. 40 × 30 m., which are almost the same dimensions as those of the Khrysolakkos cemetry (Shaw 1973 (1)) (and incidentally on the same alignment, and not on that of the palace). There are buildings around, and it may well have been used as an Agora. The Hypostyle Crypt (Amouretti 1970), with its storerooms for olive oil which seems always to have been an important part of the economy of Mallia, would have formed part of it. The only

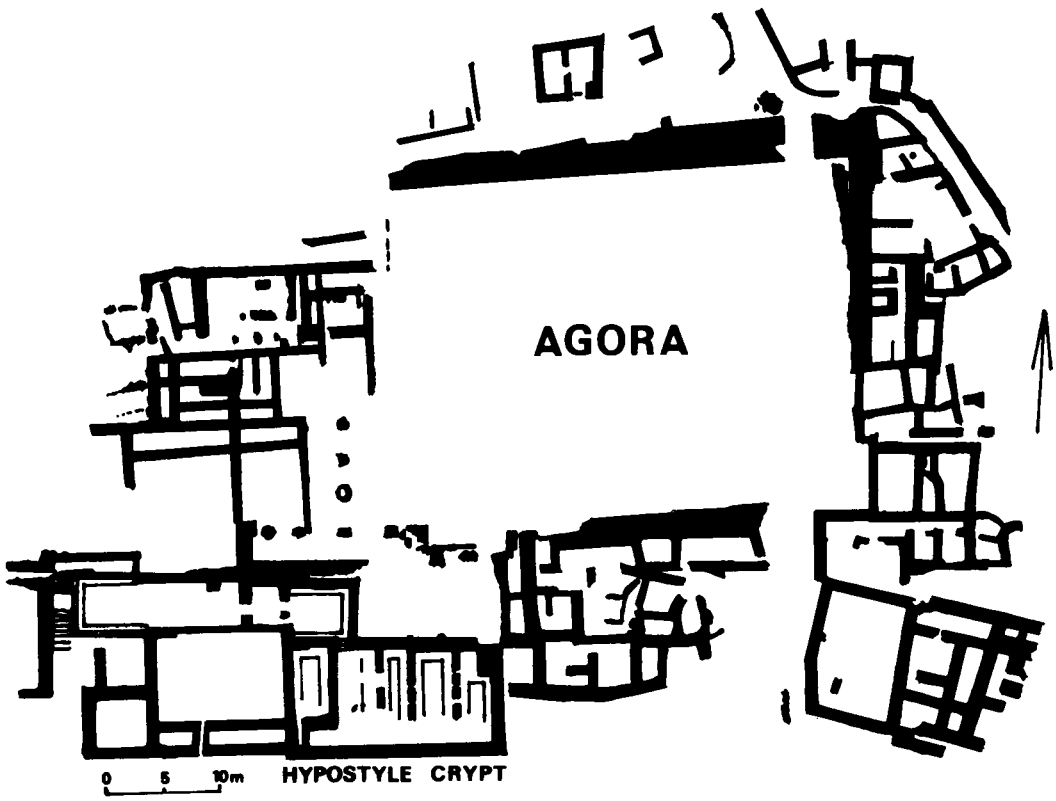

Plate 17a

Mallia: Agora

comparable space would be that in the town at Ayia Triadha, which is much later (Banti 1973); but little has been dug of the town of Knossos.

Quartier Mu (Pl. 17b) at Mallia is much more puzzling for, as Poursat says, it has evidence for so many of the functions of a palace: Hieroglyphic tablets and seals, precious contents, the earliest basin-room and storerooms, reception rooms, peristyles and light wells. The seal-worker's shop was nearby (Poursat *et al.* 1978: 25-27). The architecture of Quartier Mu is, if anything is in Minoan Crete, an agglomeration: one unit with a higgledy-piggledy design, loosely separated into three parts. Should we see it as being somehow related to the supposed Old Palace as we may imagine the so-called Arsenal on the North side of the Royal Road at Knossos

Plate 17b

Mallia: Quartier Mu

was to the New Palace—a subordinate part of the apalce, outside the main building? Or was Mallia different from Knossos and Phaistos in that it was slow to start building a standard palace around a central court, and that for a time the palatial functions were run from Quartier Mu? It may be. If so, it would mean that the palace did not come till very late, the beginning of the New Palace period, unless the two buildings worked in tandem for a time. The overall plan of Quartier Mu is hardly palatial, although so many of the building's functions and architectural elements are; it seems rather democratic, with the democracy of an Eastern bazaar, as does Fournou Korifi which is sometimes suggested as a predecessor of the palaces (Warren 1972 (1): 260-61; Branigan 1970: 48-49). In both we see the division of labour as reflected in the division of space by function, and a tight organisation moving from the inside outwards. But, if we accept the similarity between Quartier Mu and Fournou Korifi, then we see how far we are still from the conventional Minoan palace, with its organisation of a *large* amount of space around a *large* North-South central court, to which all axes are related, and with the strong sense of one predominant power.

It would seem then that the plan around a central court marks a deliberate break from such plans as Fournou Korifi or Quartier Mu. There is nothing I can find in Crete to explain how the plan was decided upon, although the Agora of Mallia shows that a similar plan could be used, on a different alignment for a different sort of public building. Development from a village square is likely, but how significant is that? The difference in scale is enormous. In terms of extant remains, there is no village square comparable. Vasiliki is our best example. In terms of society, the Minoan palaces demand a society far more hierarchical (in the original sense of the word) than that of a village. It seems much more a matter of combining a village with a precinct, the boundary being defined by what is happening inside. (Part of what was happening inside would have been religious ritual which often took place, we may suppose, on the West sides of the palaces, which is where so many of the religious rooms are. Shaw has suggested that these rooms would have been illuminated by the morning sun, which may give us an explanation for the long North-South axes of the central courts governed, as always in Minoan Crete, to some extent by the lie of the land (Shaw 1973 (2)).)

We still have to face the problems of size, complexity and organisation. The evidence is just not strong enough. We can see dimly the improving technology, the growing use of seals, the growing links with abroad, the skills—considerable—in the third millennium, the continuity of customs of burial, the beginnings of mountain shrines, the importance of grazing and hence of seasonal movement, the improved health, and the appearance of pot marks. But they are all just clues. We do not see how the jump was made from a village or proto-urban society to one centred on the palaces and/or such buildings as Quartier Mu. Who was in charge? Were the rulers chosen? Or did they choose themselves? By warfare perhaps, as at Mycenae in the sixteenth century? Are the Early Minoan II destructions and Early Minoan III abandonments evidence of struggles for power in the island? Does the Early Minoan III proto-palace at Knossos mark the appearance of the first victor, or perhaps we should go back as far as the Early Minoan IIA building below the West Court; and do the first palace buildings at Mallia and Zakro, wherever we place them, mark the last victors? How were the plans chosen? Who were the masterbuilders?

It is all murky, as are so many sudden developments for which we have only archaeological evidence. We must not try to fill the void with pompous socio-economic statements which turn out to say the *a priori* obvious or nothing at all; but I am not sure if we can say anything more. With their culture, the palaces seem to have grown out of Early Minoan Crete; and their apparent self-sufficiency in the Old Palace period underlines this.

I do not see how we may postulate foreign conquerors or immigrants to introduce the palaces, as one might label the Minoans themselves in the New Palace period to account for the Minoanizing of the South Aegean. We have no evidence, nor are there convincing parallels in design between the Minoan palaces and those of western Asia (Graham 1962: 229-33). If copper was coming from Cyprus as Crete's principal import, we can detect no influence on Crete from a then less-developed island.

There is however a generic resemblance which may give us a better idea of how the palaces came to be. The Minoan palaces, at least at the end of the Old Palace period and probably long before, held the central position in society. They were the centres of the economy and administration, and of the foreign affairs of Crete, and of the arts and crafts, and were supported by vast amounts of farm produce kept in their store-rooms and a probably fairly elaborate bureaucracy to ensure that it got there. They were among the centres of religion, probably the centres. The evidence for the mixture of religion, bureaucracy and taxation, storage and the arts is of course stronger in the New Palace period. It is so strong that it is difficult to see any distinction between sacred and secular in the buildings at all. I think there is enough from the Old Palace period to suggest that this was also true then. (In fact it is from this very consideration that Poursat raised his questions about the palatial character of Quartier Mu (Poursat *et al.* 1978).) In the role then of religion, and generally in the arrangement of space and function, Crete's palaces would appear to be at the West end of a long line of palaces, palace-temples and temples stretching to the East as far as the Euphrates and the Tigris (cf. Anon. 1972). I do not mean any direct influence: Heaven forbid in these days! I do mean that a Minoan going to the Levant or a Levantine coming to Crete would have found the social organisation familiar; which suggests in turn that the conditions that produced such buildings and their organisations were similar. But even in Mesopotamia, or Egypt, all we can point to is the combination of political and religious power, natural resources and the skill to exploit them. The change that created the ruling class is elusive, as is the part of religion; but it is clear that royalty and/or priesthood, as the epiphany of the divinity, could manage society, perhaps even easily. Indeed in Crete they may have been even more successful than on the Levantine mainland, which may be part of our difficulty in identifying the religious: at Knossos, for instance, the proportion of space given to storage is far greater than in any Eastern palace (Childe 1954: 165).

If we accept this argument, at least in principle—and I am quite aware how difficult it is to substantiate—we need not be diverted because the buildings in Crete are known as palaces. Evans started digging at the residence of King Minos when Queen Victoria was still alive; and the *pax Britannica* and the *pax Minoica* soon fused,

with the help of Thucydides and Pericles. Nor should we be diverted by talk of Crete's having Europe's first civilisation, a cliche that has bedevilled East Mediterranean archaeology. The Minoans did not care if they were Europeans or not, even if it was a distinction that mattered in the Aegean in Classical times.

We may now come to the question: why was Crete generally self-contained in the Old Palace period? Once the changes had begun, they would have gained momentum fast. Demand would have created demand. Builders, craftsmen, suppliers of building materials, farmers, all were needed with their products. The speed of development that we infer is no surprise. The progress was self-renewing and produced a steadily increasing population into Late Minoan I, good health, some remarkable art and a continuing prosperity in the New Palace period which does not seem to have been impeded by the disasters of the end of the Old Palace period; but Crete's apparent lack of contacts with the South Aegean is surprising. A few vases were exchanged. There was some influence in pottery. Cretan raids have been suggested. They are quite possible; but the reason may be that Crete was preoccupied with its own troubles and strife and had been since Early Minoan times (Alexiou 1979). Sites had been chosen for settlement for being defensible; at Pyrgos two cisterns and a tower were built, and there may have been a bastion earlier at Fournou Korifi; settlements were destroyed, especially at the end of the Old Palace period; and the ceramic groups show up regional differences. With the New Palace period came a homogeneous culture. We may assume that Knossos had won, Alexiou suggests. Only then could Crete, her own problems resolved for the time being, turn to the Aegean, most of which was quickly drawn into a Minoan sphere of influence and way of living.

We should now turn to our title question. Why was Crete different? I hope I have shown that it was not quite so different as is often thought, that war probably was a problem, and that there were destructions and abandonments. And yet the old view implicit in the question is right: Crete's achievements in the early second millennium are far removed from the contemporary life of Mainland Greece, and fairly far from that of the islands. The founding of the palaces is the immediate answer, but that is not enough. To help explain the palaces we have looked at the rich food supplies, better health, better technology, the probable supply of materials such as copper from abroad and the power of religion. From this mixture, somehow, came rulers who ordered the buildings and put the organisations together. These are all advantages that Crete enjoyed, and each would have affected the others, severally and together. But something must have changed also on the Mainland. Seeing Building BG and the House of the Tiles at Lerna and the Early Helladic II culture in general, we could be forgiven for expecting something monumental or palatial in

Middle Helladic times. What then were the Mainland's disadvantages in Early Helladic III and Middle Helladic? The presumed arrival of newcomers may have been one. The damage and disruption and enfeeblement of the population may have been far worse than we imagine. The craftsmen may have all been killed, and technology destroyed; but the evidence does not bear that out (cf. Howell 1973). It is hard to explain a recession. It may be that it was that the Mainland was not exposed to the Near East and Egypt, as Crete was, and so never learnt the idea of palaces and some of the tricks of technology. But Crete was a nearer example for the Mainland, and was on the whole ignored. We must conclude that society was not in a position to want or need palaces on the Mainland, just as in Crete it did want them or need them.

If there were any mysterious Oriental ideas to help the Cretans, I imagine they were quickly double-axed to shape, Crete being rich, self-sufficient except in tin and probably copper, conservative and self-contained and an island difficult of access on the way to nowhere. I still do not see quite how the palaces came about, and am unhappy with the determinism of saying that they just happened without trying to say why. The evidence of Mallia at least suggests that it was a complicated, slow and irregular change there. I think the central court is all important as the core of the palace precinct, around which and in which all its business took place. The Minoans loved formality and would have found it the formal expression of the unity of their life; but, till we excavate an Early Minoan example, we still do not know how the idea was formed. What we do have is already from Early Minoan II the evidence of distinctive wealth and, I think, of war and of a ruling class who could have harnessed the skills and energies of Crete and ordered the palaces, which may well have begun in the succeeding phase, with predecessors of some sort perhaps already in Early Minoan II.

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BSL: *Bulletin de la Societé de Linguistique de Paris.*
JIES: *Journal of Indo-European Studies.*
TPhS: *Transactions of the Philological Society.*

In transliterating Greek names we have followed what is familiar rather than what is always consistent.

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